WORD

JOURNAL OF THE LINGUISTIC CIRCLE OF NEW YORK

EDITORS

WILLIAM DIVER ANDRÉ MARTINET URIEL WEINREICH

VOLUME 15

1959

Published by

THE LINGUISTIC CIRCLE OF NEW YORK

New York, N. Y.

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VOLUME 15, NUMBER 1 APRIL 1959

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W. HAAS

Relevance in Phonetic Analysis

I. Introduction

The interest in method is rather frowned upon by some linguists. Where there is so much to do they would rather get on with it. The attitude seems sensible so long as it is possible so to get on. But this is not always the case. And when our work fails to advance, or begins to yield embarrassing results, before very long we are bound to ask what exactly we have been doing, and perhaps doing wrongly.

One might almost say it was in a situation like this that modern phonetics was born. Increased accuracy of observation seemed to threaten it in its foundations. Having discovered that "the sounds of speech present an almost infinite variety," phoneticians began to find it increasingly hard even to make an inventory of them, much less reduce them to a system.[1] More and more, they were forced to recognize that phonetic analysis was not, and had never been, a matter simply of recording observations: it was founded in *selective* observation. The more sensitive the phonetician's ears and eyes and machines, the more they required the guidance of *criteria of relevance*. Without such, the investigator was liable to be "swamped by his mass of observation";[2] the "flow of speech" presented itself as "a continuum capable of being divided into any number of parts, all of them equally important."[3] Random advances made in degree of exactitude could only "clutter up his record" for him;[4] this, with "details which in themselves had no value."[5]

It is important even now to recall those early days of "phonology" or "phonemics," of "practical" or "functional" or "structural" phonetics. Some of their fundamental insights have tended to get lost in these later

[1] Cf. W. F. Twaddell, *On Defining the Phoneme*, 1935 (=Language Monograph 16), reprinted in *Readings in Linguistics* (ed. Martin Joos), Washington, 1957, p. 55.

[2] Edward Sapir, *Language*, New York, 1921, p. 58.

[3] N. S. Trubetzkoy, *Grundzüge der Phonologie*, Prague, 1939 (Travaux du Cercle Linguistique de Prague 7), p. 16.

[4] Leonard Bloomfield, *Language*, New York, 1933, p. 84.

[5] Ferdinand de Saussure, *Cours de linguistique générale*, Paris, ₄1949, p. 77.

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days—lost not so much by the momentum of the discipline's own development as for extraneous and doctrinaire reasons. What were those insights?

Chiefly, they were three:

(i) that without applying selective principles we have no chance even of arriving at anything like a limited inventory of phonetic elements;

(ii) that selecting and grading—operations always unavoidable but which had hitherto been tacit and arbitrary—should be made explicit and rational; and that a rational selection of portions and features of speech will refer to their communicative value, i.e. their occurrence as constituent elements in the structure of meaningful utterances;

(iii) that by use of such carefully graded criteria of relevance, much more than a mere inventory is derived from the continuous "flow of speech." What had been represented as mere "sound material," parceled out under the rubrics of a phonetic table, revealed itself now as a complex organization, with grades and ranks and complementary roles—the organization of a "signaling unit" of which we could almost say that it is active, as a whole, in every utterance.[6]

The urgency and general nature of the task has not been in dispute; but the principles applied to its solution have come to differ in many ways. My intention in what follows is to confront what I take to be the two main types of such principles.

II. Semantic Relevance and Distributional Criteria

Semantic Criteria

The selective criteria proposed by all the earlier phonologists—de Saussure, Sapir, Trubetzkoy, Bloomfield, Hjelmslev, Firth—involved considerations of meaning. The data to which they applied their phonetic analysis were *signs*, i.e. meaningful utterances or meaningful parts of such (sentences, phrases, words, morphs); they were never mere "stretches" of speech. For, their principal analytical method ('contrastive substitution' or 'commutation') worked with *differences of meaning*: the given signs came to be analyzed phonetically by correlating their differences of form with differences of meaning. When Bloomfield[7] contrasted *pin* : *tin* : *sin* : *fin*, he picked out *p*, *t*, *s*, *f* as mutually substitutable for one another in the unit-frame *–in*, and as relevant, because these sounds distinguished signs of different meanings. We are asked to select what has diacritical power.

[6] "... où les éléments se tiennent réciproquement en équilibre selon des règles déterminées" (de Saussure, *Cours*, p. 154).

[7] *Language*, p. 78.

By this method, all that we come to know about the phonetic constitution of speech is relevant. The rest, in its almost infinite variety, is left indefinite; it remains merged in the concrete form of the sign as a whole. *Semantic criteria*, as we may call those applied in procedures of contrastive substitution or commutation, are used exclusively for picking out the grain; no such thing as a phonetic chaff appears within the scope of this kind of analysis.

Yet, *nothing is specifically excluded*, and this for two reasons: (i) because the *whole* of any sign comes within the scope of commutation-techniques,[8] and (ii) because semantic criteria admit of *degrees* of relevance. Both these points seem to have been overlooked at times. Briefly, what I wish to stress is this:[9]

(i) To select an element by commutation is to state a syntagmatic relationship of it to the *whole remainder* of the sign. If we said, simply, as some do say, that the commutable element occurs in "an environment of other elements," we should be in danger of missing much that is important. Commutation works with signs and sign-frames; *mere* environment is not enough for it. (We do *not* consider "contrasts" like *pling* : *bling* : *tling* : *dling*, as cut out from, say, *tippling*, *grumbling*, *settling*, *cuddling*.)[10] This means that the environment of a commutable element can be indicated as a whole, viz. as "*the remainder of the sign*," so that its description need not be supposed to be exhausted by an enumeration of just such "other elements" as happen to have been picked out in some preliminary transcription. It is especially important to remember that amongst the elements included in that "remainder of the sign" there are some that are not commutable at all, and which are yet supremely relevant for the phonetic characterization of a language. Such constituents of the whole sign, though not commutable themselves, are yet isolated by commutation—namely, the commutation of other elements. For example, when we have obtained the segments *p*, *i*, *n*, of *pin* by commutation, we have also obtained the syllable contour CVC of the word: we have isolated it as the relatively constant remainder—the determinant frame, within which the commutations of the

[8] I am here using Hjelmslev's term 'commutation' in an extended sense which seems to be gaining currency, namely, (a) without implying any particular theory of meaning, and (b) applying it to the contrastive substitution of mere features of speech as well as of sounds.

[9] A fuller treatment must be reserved for another occasion.

[10] On the "fatal geographical term environment," see Paul Diderichsen, "The Importance of Distribution Versus Other Criteria in Linguistic Analysis," *Reports for the Eighth International Congress of Linguists*, Oslo, 1957, pp. 135ff. See also my paper, "On Defining Linguistic Units," *Transactions of the Philological Society*, 1954, pp. 54ff.

segments take place. The fixed stress patterns, by which some languages mark their words, are another example of such *determinant elements*; they are isolated as remaining constant in relation to commutations within the word.[11]

(ii) Commutation techniques are essentially operations of grading. The constituents of a sign come to be placed on a continuous scale, on which there is room for anything we care to notice—from the most to the least important.[12] For, both diacritical and determinant power are matters of degree. They are a function of the relative independence, regularity, and frequency of an element's occurrence in either of these roles.

Thus, in using semantic criteria we only *select* elements; we discard none. Corresponding to the two aspects of commutation—variancy of the commutables, on the one hand, and relative constancy of their frame, on the other—there are two principal modes of relevance: diacritical power and determinant power. By commutation, elements will be selected as being relevant in either mode, and subsidiary criteria will distinguish varying *grades* of such relevance amongst them.

The "phonetic system" of a language will then appear as an organization, in which every element, besides *being* a part amongst others, has a part to *play* in the constitution and discrimination of meaningful utterances.

Distributional Criteria

All this was changed, when, from the philosophic side, 'meaning' came under suspicion. Reference to signs and to differences of meaning now became objectionable. The most radical expression of this is wholly to replace semantic criteria by *distributional*. Instead of taking signs for our data, we are then supposed to work with mere "stretches of speech"; and instead of operating with differences between the *meanings of signs*, we are supposed to consider exclusively relations between the *distributions of sounds*, i.e. to examine the part–part relations of phonetic elements to one another, rather than the part–whole relations of the elements to the signs in which they occur. It is supposed to be all right, for example, to take account of the relation between *p* and the environment *–in*, but never (in phonetic analysis) to concern oneself with the relation between *p* and the *word* "pin" or the *word-frame* "–in". The change is decisive. It entails (i) a very great complication of the phonetician's selective procedures, and (ii) to a signifi-

[11] It is the merit mainly of J. R. Firth, and of work done at the London School of Oriental and African Studies, to have kept us alive to the importance of this aspect of phonetic studies.

[12] Cf. André Martinet on the non-exclusiveness of phonology (*Phonology as Functional Phonetics*, London, 1946, pp. 9, 21, 40).

cant extent, a return to the prejudicial arbitrariness of a phonetics without criteria of relevance.

If today we can see more clearly what is involved in a non-semantic approach to phonetic analysis, we are indebted to those who have worked it out consistently, mainly to Bernard Bloch—to his paper "A Set of Postulates for Phonemic Analysis" and its sequel on "Contrast."[13]—True, Bloch's postulates for phonemic analysis are expressly declared *not* to be "intended to delineate procedures," or "to constitute a list of practical rules to be followed step by step in one's work with an informant." Obviously, a set of postulates is not a schedule of procedures. It is also true that "appeals to meaning" are expressly admitted as "practical devices" and "a shortcut." But it does not seem to have been Bloch's intention to supply us with a loop-hole for evading the rigor of his scheme at our pleasure. For we are further told that "the methods of analysis by which linguists usually proceed in arriving at the phonemic system of a dialect are implied in these postulates and can be justified by them." The set of postulates is intended to bring out the "tacit assumptions" of a method of analysis.[14] It is clear, then, that "appeals to meaning," though almost inescapable, are not regarded as indispensable, and are admitted only as something like a trick of the trade—a time-saver. The procedure of phonetic analysis, to the extent to which it is capable of rational justification, is supposed to be stated in terms of distribution.

It is not surprising then that many linguists (and Bloch amongst them)[15] have tried, in their *procedure* of analysis, either to dispense with appeals to meaning altogether or, at any rate, to restrict them to some precisely defined task and stage in the progress of analysis. The following statement by Z. S. Harris (quoted from a section outlining a "schedule of procedures") may be taken as representative: "In both the phonologic and morphologic analyses the linguist first faces the problem of setting up relevant elements. To be relevant, these elements must be set up on a distributional basis. . . ."[16]

Hence the importance of realizing, first of all, what exactly is involved in adopting an *exclusively* distributional approach.

(i) COMPLICATED PROCEDURE. Whereas, formerly, a single semantic contrast had been sufficient to make a contrastive pair of phonetic constituents

[13] *Language* XXIV (1948), 3–46, and XXIX (1953), 59–61.

[14] "A Set of Postulates . . .," p. 5.

[15] Cf. his "Studies in Colloquial Japanese: Phonemics," *Language* XXVI (1950), 86–125; reprinted in *Readings in Linguistics* (cf. footnote 1).

[16] *Methods in Structural Linguistics*, Chicago, 1951, p. 7.

relevant, it is now necessary, first, to pick out arbitrarily two different segments or features, and then to compare their whole distributions, i.e. the total ranges of their environments, before we can know whether the two differ in any relevant way—a truly forbidding task.

In order to conclude that the difference, say, between *p* and *b* is relevant in English, we are now not allowed to use evidence such as the contrasts, in both form and meaning, between *pear* and *bear*, *pet* and *bet*, *rope* and *robe*. For the conclusion to be 'justified', we must now:

(a) be lucky enough to have distinguished the two sounds without semantic aids (i.e. even if all the sequences examined were like *bling*, *pling*, *tling*);

(b) have listed *all* the environments of each in terms of other elements (these, too, having come to be available through sheer meticulousness of observation);

(c) have compared the environments of the two sounds, and found that only some are common to both, while others are specific to each (if all their environments were common, they would be "free variants"; if none were common, they would be "automatic variants");

(d) have found that there is no general definition of the common environments to distinguish them from the specific. (If such a definition were possible, then, presumably, we should have two systems, and the two sounds would be judged to be "free variants" in the one, and "automatic variants" in the other.)[17]

Fulfilment of all these conditions is supposed to provide us with an equivalent for "contrast" or "commutability" in terms of distribution, i.e. "without the customary appeal to meaning."[18]

No one will deny the theoretical interest of such statements of distribution, but not a few will doubt that they can serve the task which is here assigned to them.[19] Furthermore, even if the complicated procedure itself is considered to be feasible, it would still be open to the serious objection of

[17] I must confess that I fail to understand why two sounds which occur in a definable range of common environments (while they do not share others) should be precluded from contrasting with one another within that range—that is, in the old sense of 'contrasting', for which we are here seeking an equivalent in terms of distribution. Surely, it is not enough to say that it has turned out to be so in many cases. For this is not a question of empirical fact, but a question of understanding the logic of the terms we use.

[18] Cf. B. Bloch, "Contrast," p. 60.

[19] Cf. Eli Fischer-Jørgensen, "The Commutation Test and Its Application to Phonemic Analysis," in *For Roman Jakobson*, The Hague, 1956, p. 145. I shall try to show (Section IV below) that distribution studies of another kind might well be more appropriate to the task.

(ii) ARBITRARINESS. Whereas semantic criteria are applied to unanalyzed signs, and enable us to pick out directly what is phonetically relevant, distributional criteria require an *antecedent* phonetic analysis to supply them with the elements whose distributions are to be compared. But with that antecedent analysis we have returned to naive phonetics. Not surprisingly, we are told, once more, that in order to avoid missing what may be important, we must try to notice everything. The intrinsic arbitrariness of the procedure is to be cured by wastefulness.

It has been claimed that some such initial arbitrariness is unavoidable in every approach to phonetic analysis, whether it relies on distribution or on semantic contrast. If we refer to contrasting *signs*, then—we are told—we must have identified and recorded these signs in some way, just as, when referring to the distribution of sounds, we must have identified and recorded the sounds. And it is claimed, further, that for the purpose of phonetic analysis, signs cannot be effectively identified except in terms of their constituent sounds: they have to be recorded ("reduced to writing") on the basis of a provisional phonetic analysis. This seems plausible; but the operative word, here, is "provisional."

Clearly, whether a descriptive statement is provisional or not, depends entirely on how we treat it, after it has been made. An initial transcription of utterances in terms of sounds and sound features can only be said to be provisional, if our subsequent procedures of phonetic analysis are capable of replacing it. Now, distributional techniques cannot do this. On the contrary, they must work on the assumption that the initial phonetic description has been complete and adequate. A mere transcription, then, so far from being provisional, turns out to be *prejudicial.*

How, then, are we to make sure of the provisional status of an initial transcription? Only, I suggest, by keeping it strictly to its limited task—the task, namely, of helping us to identify and record *whole meaningful utterances*. Its task is *not* to identify *sounds*. Naturally, in labeling signs with some of their phonetic properties, we do incidentally perform some kind of analysis. But if this incidental analysis is treated as corrigible in every item, then it will still be true that the subsequent phonological analysis is performed on unanalyzed signs.[20] Distribution techniques, however, are

[20] In fact, total identification-labels—logograms or some conventional orthography like English spelling—would be sufficient for a phonetic analysis which operates with signs. Working with such labels, we might state, for instance, that *so* and *though* are more similar to one another than either is to, say, *son* or *tough*, and that the two, *so* and *though*, seem to contrast with regard to their initial segments, which—as we should decide only then—may be transcribed phonetically as /s/ and /ð/ respectively.—Of course, there is no reason why a more consistent transcription should not be used

confined to the data picked out by the first transcription. There is, in fact, no such thing as distributional *analysis*. There is only the distributional *grouping* of data which have been obtained by mere phonetic transcription.

This amounts to a complete reversal of the original method of the earlier phonologists. The relation between "phonemics" and "phonetics" is now given a new and ominous complexion. For, clearly, that antecedent "purely phonetic" transcription has no criteria of relevance at its disposal, either distributional or semantic; it is a return to precisely those arbitrary compilations and "cluttered up" records from which the earlier "phonologists," with their "practical" or "functional" phonetics, had tried to save us. Distributional criteria require that kind of arbitrary and inflated record to work on—a mass of chaff along with the grain. For their function is to discard chaff rather than pick out grain: mainly, to *disregard* the less important (viz. differences of elements "in free variation" or "in complementary distribution") rather than *note* the more important (viz. similarities with regard to contrastive features).

The assumption is, of course, that in the end what remains *is* the grain, and that it is *all* the grain. This assumption is obviously unjustified. For the distributional thrashing techniques work on a random harvest. We have come full circle: from the original criteria of phonological relevance, which were intended to put an end to uncontrolled observation, we are led back to rely, once more, on the completeness of a bloated phonetics. This is sheer illusion. When our basic data are assembled in an arbitrary manner, even the most formidable effort to record "every discernible feature"[21] may easily fail to take in what is relevant; and no amount of subsequent thrashing can then produce it.

This is admitted. "Over-differentiation," we are told, "can be discovered and corrected" by distributional techniques; but "no systematic procedure will discover and correct under-differentiation."[22] This—unless, indeed, we are prepared to abandon the distributional approach and apply

from the beginning. On the contrary, it has advantages to offer, provided only that we remain aware of its dangers and treat it as provisional, i.e. treat the transcriptions of signs as essentially non-analytical global indications, as identification labels of the signs, *not* of their constituent phonetic elements. The latter have to be extracted in a less arbitrary manner.

[21] H. A. Gleason, Jr., *An Introduction to Descriptive Linguistics*, New York, 1955, pp. 175; how alarming a development this is, appears clearly from the fact that about one-half of Bloch and Trager's *Outline of Linguistic Analysis* (Baltimore, 1942) is taken up by "a general survey of the whole domain of speech sounds"—by "chance observations of speech sounds," as Bloomfield had called them (*Language*, p. 127).

[22] Gleason, *Introduction*, pp. 174–261.

semantic criteria after all. "Omissions," we are told, "can be brought to light . . . by a careful correlation of phonetic differences with differences of meaning."[23] But then, the semantic criteria which, here, are brought in again, as a late safeguard and remedy, could have been applied more effectively to *prevent* failure; they could have been entrusted with the whole work of selection from the beginning.

A Compromise

In order to avoid some of the drawbacks of a distributional approach, many linguists have relaxed their partiality for it. They have accepted a restricted use of semantic criteria: for the purpose, namely, of supplying distributional techniques with the basic data. At the same time, reference to meaning is made to look more innocent. Bloomfield, at one time, though himself suspicious of meaning, felt bound to say: "Only by finding out which utterances are alike *in meaning*, and which ones are different, can the observer learn to recognize the phonemic distinctions."[24] Now, we are told, in effect, that "only by finding out which utterances *sound* alike to the native speaker and which sound different, can the observer learn," etc.[25] But this turns out to be merely a new disguise for the same old thing. Obviously, the native speaker's reactions are determined by his knowledge of the signs, meanings, and grammar of his language.[26] And if he is not sophisticated enough to know that this is expected of him, when he is questioned about sounds, then "sooner or later it is almost always necessary that the analyst acquire the ability to make for himself judgments of 'sounding same' and 'sounding different'." And he will do this, we are told, "*not on any basis of general phonetics.*" Rather, "he must to some extent learn the language with which he is working"—which means, in fact, that his judgment of the sounds must come to be controlled by a knowledge of signs and meanings.[27] Surely, the time is ripe for dropping the disguise altogether.

The work of analysis cannot be evaded. A statistics of native speakers' reactions, helpful though it may be, is no substitute for analysis. After all, the question remains: What are the principles of selection that operate in those reactions? Phonetic analysis is an attempt to trace these principles;

[23] Bloch and Trager, *Outline*, p. 39. Similarly, in his rather eclectic and unsystematic treatment of phonetic analysis, Gleason, *Introduction*, pp. 261f.

[24] Bloomfield, *Language*, p. 93.

[25] Z. S. Harris, *Methods*, p. 20.

[26] See Dr. Fischer-Jørgensen's criticism in "The Commutation Test . . .," p. 144.

[27] The admission is C. F. Hockett's in his *A Manual of Phonology*, Baltimore, 1955 pp. 146f.

it aims at raising them to the level of conscious analytical operations. If, having done this, we recognize that the operations which are implicit in anyone's perception of his native tongue do involve "reference to meanings," then surely we must also realize how entirely unjustifiable are all efforts to avoid the use of semantic criteria.

Even that we should restrict their use in the way here proposed—namely, to preparing the ground for distributional techniques—is a suggestion which has little to recommend it. It is true that commutation will here supply us with a basic inventory of contrastive sounds (allophones) which is much more adequate to the task than an arbitrary collection of "every discernible feature" could ever be. But if, later on, we decide to confine ourselves to distributional techniques, the two fundamental difficulties characteristic of them will still be with us. The difference will only be that they will affect the procedure of determining the phonemic *type* or *class* of the sounds rather than the procedure of gathering the sounds themselves.

The allophones we obtain by commutation are, of course, indefinitely varied and numerous, and our next task must be to group this enormous variety in a few classes or types. Clearly, the questions we are asking here cannot be deemed to have been settled by our first provisional transcription. If two sounds have been transcribed in the same way—e.g. by *p* in *pill* (: *bill* : *mill* : *fill* : *kill* . . .) as well as in *pitch* (: *ditch* : *rich* : *witch*)—we must still ask whether they belong to the same phoneme. It is in trying to answer such questions that we meet the same difficulties again.

(i) Complicated Procedure. A continued use of semantic criteria would enable us *directly* to pick out the recurrent contrastive features which *characterize* a phoneme in its many allophonic variants. We should find that many contrasts, each involving different signs and different sounds, can all be interpreted in terms of the *same* contrastive features (e.g. the contrasts *pill* : *kill*, *pear* : *care*, *spare* : *scare*, *rim* : *ring*, *slim* : *sling* etc., all in terms of the contrast 'labial': 'velar'). This means that very many different distinctive sounds come to be characterized by similar distinctive features (e.g. characterized as 'labial' or as 'velar')—and, ultimately, that each comes to be characterized by one of a few *bundles* of such features—which bundle determines its phonemic type.

The procedure, here, consists in correlating semantic contrasts with *phonetic features* (not merely with sounds). Now, if this way of selecting what is relevant or typical about distinctive sounds is regarded as objectionable, then the task of grouping allophones will be entrusted, once more, to the distributional thrashing techniques. In order to establish phonemes, we shall then try to discover what allophonic *differences* are *irrelevant* for

the purpose, rather than what *similarities* are *relevant*. We shall try to eliminate the chaff, rather than pick the grain. The procedure will again be the extremely complicated and dubious one of comparing "total ranges of environments" (see above)—only that the object will now be to discover which of basic distinctive sounds do not contrast with one another.

(ii) ARBITRARINESS. It is admitted that considerations of distribution are not enough. Intrinsic phonetic *similarity*, too, is appealed to. It is considered to be a subsidiary criterion for assigning allophones to the same phonemic class; this happens in the many cases where criteria of distribution admit of alternative groupings (e.g. on account of so-called "multiple complementation"). Again, *dissimilarity* is regarded as a sufficient condition for assigning allophones to different phonemes; this, where purely distributional techniques would lead to absurd results (such as the notorious grouping of English /h/ with /ŋ/, or of /ð/ with /ŋ/, and almost of /ð/ with /θ/).

Thus, there are intrinsic dissimilarities which are too important to be disregarded, when allophones are grouped into classes, and similarities which are important enough to determine the grouping in cases of doubt. But then the question arises: What is to count as important?—similarity, and dissimilarity, *in respect of what features?*

This question cannot be decided by mere inspection of the sounds themselves.[28] Why should the dissimilarity of "dark *l*" and "clear *l*" in English not be important enough for assigning them to different phonemes, and, on the other hand, the dissimilarity between /h/ and /ŋ/ be important enough? If we found /h/ and /f/ in complementary distribution, could we decide, by mere inspection of these sounds, that they are dissimilar in an important way? Again, if we found /t/ in complementary distribution to both /p/ and /k/, could we decide on general phonetic grounds which similarity is more important, its being 'frontal' like /p/ or its being 'lingual' like /k/?

If semantic criteria are applied, i.e. if the contrasts of sounds are interpreted in terms of their distinctive features, then such questions are easily answered: What is important is similarity, or dissimilarity, with regard to distinctive features. But if we refuse to apply semantic criteria, then there is no alternative but to fall back on the dogmatic decisions of a "general phonetics."

Our objection here is not to "phonetic realism"; it is to an *arbitrary* realism. It can be shown, I think, that many of our phonetic descriptions have been graveled through having been uncritically forced into the

[28] de Saussure, in the quoted place, says "pas de valeur en eux-mêmes."

pigeonholes of a dogmatic general phonetics. Within such a pre-established framework (IPA, for instance), the very symbols we use for marking the basic allophones are liable to become a hindrance. At the very beginning, when they should mark mere substitution-counters, the symbols are apt to be taken dogmatically to have reference to fixed combinations of certain phonetic features (*p* = voiceless labial plosive, *f* = voiceless labio-dental fricative, etc.), and then immediately a dangerous bias is put in the way of a functional phonetic analysis. Warnings against this source of error have not been rare.[29] But a distributional approach has no effective safeguard against it. Its decisions as to what is similar, and what dissimilar, are imposed by some pre-ordained description of the contrastive sounds.

If it was necessary to rely on criteria of relevance for selecting *sounds* as

[29] I cite a few examples, from scholars most of whom have no special preference for distributional techniques.

Yuen-Ren Chao, "The Non-Uniqueness of Phonemic Solutions" (reprinted in *Readings in Linguistics* [cf. footnote 1], p. 53, about "typical phonemes": "A European would group [p] and [p'] under one typical phoneme, as against [b], while an unsophisticated Chinese phonetician would most likely group [p] and [b] under one typical phoneme as against [p']." Clearly, the meanings of the three symbols are different in the two cases, and the task of phonetic analysis is to say what the meanings are in the one and what in the other.

Or W. S. Allen (*Phonetics in Ancient India*, 1953, 59f.) on the early Indian characterization of both [a] and [h] as 'glottal'. This makes perfect sense to us, if we free ourselves of the bias, on the one hand, for a typical phoneme *h*, in which 'glottal', being joined to 'fricative', must characterize a specific consonant, and on the other, for a typical phoneme *a* which must be 'low' and 'open' and a specific vowel. (In this connection, see also R. K. Sprigg on the difficulties of having to operate with symbols for "typical speech sounds," in his paper "Junction in Spoken Burmese," *Studies in Linguistic Analysis*, London, 1957, p. 108).

Again A. Martinet (*La description phonologique*, Geneva—Paris, 1956, pp. 35f.), having surveyed neat physiological differences "comme entre occlusives momentanées et continues fricatives", proceeds to tell us: "il ne faudra pas s'étonner si l'on trouve, groupées en une seule et même unité distinctive, une momentanée et la continue correspondante." But this means that as soon as we tackle the task of describing any particular language, we must be prepared completely to re-draw that original map of neat physiological distinctions.

A telling example of the nuisance of accepting a pre-established map of sound types in the first place is found in treatments of Japanese, where the use of the dogmatically interpreted symbols *h*, *f*, *x* induces us to distinguish three types of fricative: 'glottal', 'labial', 'prevelar' (*plus* a somewhat problematic fourth, *h'*, 'glottal palatalized')—with the result that within these pre-conceived rubrics no adequate phonetic description is possible (cf. B. Bloch, "Studies in Colloquial Japanese IV: Phonemics," in *Readings in Linguistics* [cf. footnote 15], pp. 336, 340).

Many examples of the distorting influence of a dogmatic general phonetics can also be found much nearer home, only that sheer familiarity with the distortion makes it here more difficult to recognize it as such and to keep an open mind for alternatives.

significant, such criteria should be equally necessary for selecting the significant *features* of the sounds.—Is the 'non-sibilant aspiration' which the sound designated as *φ* or *f* in Japanese shares with that designated *h* neither more nor less important than, let us say, the 'labialization' or 'neutral tongue-position' which it shares with *p*, or than the 'friction' which it shares with *s* as well as *h*? Take the 'apical articulation' which, in Southern English, *r* shares with *d*, the 'friction' it shares with *v*, the 'concavity of the tongue' it shares with *z*, the 'post-alveolar constriction' and 'neutral jaw-position' which it shares with *j*, the 'voicing' it shares with any of these, etc.—are they all equally important? Is the occlusion of an English *n* as significant as its 'nasality' and 'alveolar constriction'? And would any decision here also apply to the French *n*? Again, in some English *p*'s, are occlusion, plosion, aspiration, neutral tongue-position, voicelessness, non-nasality, labiality, etc., all of the same significance? It is true that as long as we remain entrenched within a preconceived scheme of sound types (such as IPA), we are safe from being "swamped" by the mass of possible observations; but we are also denying ourselves any power of rational choice amidst the vast range of the phonetic characteristics of sounds. Rational analysis is abandoned, and dogmatic decision takes its place.

III. The Use of General Phonetics

What then is the proper function of general phonetics? It seems to be mainly as an ancillary discipline—ancillary (i) to phonetic analysis, and (ii) to phonetic comparison.

(i) The Dimensions of Phonetic Description. A general physiological or acoustic study of speech is indispensable for phonetic analysis. It provides a frame of reference in which the fleeting and apparently unconnected auditory impressions all find their respective *identifiable places*—the spatial framework, namely, of articulation (or, alternatively, of the acoustic spectrogram). Without such ultimate reference, our characterization of phonetic facts could never end. After the characterization of the many and various sounds by their features, we should have to go on to ask for a characterization of the many and various features. From such infinite regress we are saved by the fact that we can *indicate* the features with sufficient precision—namely, as occupying certain regions of "articulatory space." But—and this it is important to remember—to supply a frame of reference is not to supply its occupants. To *fill* the frame is the task of

selective criteria of relevance. General phonetics must be kept to its ancillary role.[30]

What we need for indicating and describing the elements of speech is a number of *phonetic dimensions* (in addition to the three geometrical dimensions of articulatory space). We require dimensions such as 'degrees of opening' or the 'tract of articulatory positions', the 'voiced–voiceless' dimension, etc. These are provided by general phonetics. It is true that without such a frame of reference no precise description is possible; but it is also true that the frame itself cannot tell what is worth describing.

The dimensions of general phonetics, though each has end-points, are continua, more like the axes of co-ordinate geometry than like measuring rods. Only the selective principles of phonetic analysis can provide the relevant measurements and divisions. As the system of the three geometrical dimensions supplies us with a means of locating and measuring all sizes of physical object, but does not tell us what sizes there are, so the phonetic dimensions of 'articulatory space' are receptive of whatever phonetic elements there may be, but do not tell us what elements there are. Here, moreover, even the choice of the dimensions most suitable to use will often be determined by criteria of linguistic relevance, some being more important in one language than in another.[31]

General phonetics, then, may be said to be the science of the dimensions of phonetic description. This is its use for phonetic analysis. Its *abuse* consists in prematurely and dogmatically transforming the system of mere dimensions (or variables) into a filing cabinet stocked with determinate elements. On the other hand, something like a general phonetic inventory *may be derived* from the descriptions supplied by phonetic analysis, and then find a legitimate use in the comparative or typological study of different phonetic systems.

(ii) PHONETIC ANALYSIS AND PHONETIC COMPARISON. The two must be clearly distinguished.

The comparison of different phonetic systems can only be as sound as the analysis that has given us the systems. Analysis is obviously the primary task. If it were not taken as such, if from the very beginning we allowed it to be guided by some general scheme of typical elements, then, from the

[30] Cf. de Saussure on "discipline auxiliaire" (*op. cit.*, pp. 56, 65). See also L. Hjelmslev, "Structural Analysis of Language" (*Studia linguistica* I [1947], pp. 69f.).

[31] No doubt other disciplines and interests—medical or educational—will apply their own criteria of relevance and fill the same "articulatory space" with other objects. Frequently, too, they will choose other dimensions for accommodating these objects. We may think here, for instance, of the special distinctions made for the purpose of lip reading.

very beginning, both phonetic description and comparison would be prejudiced in an arbitrary manner. If every language were to be confronted with a prepared list of "possible elements," we should be tempted to ask: "*Where are* the plosives, the fricatives, the nasals, etc.?" as if there had to be such: in much the same way as, under the tyranny of a "Universal Grammar," grammarians were tempted to ask: "Where are the verbs, the nouns, the adjectives, etc.?" Even if we were to be more cautious, asking merely: 'In this particular language, *are there* plosives, fricatives, etc.?"—much as the more cautious "general grammarians" (like Jespersen) asked, "Are there verbs, nouns, etc.?"—we should still be prejudging the issue by admitting no answer except "yes" or "no," thus precluding ourselves from discovering other elements and, perhaps, even using other and more adequate dimensions for the description of such elements as there are. The only appropriate question for an original analysis to ask is obviously neither "Where are ——?" nor "Are there ——?" but: "*What are* the elements? And how are they best described?"

Analysis, then, must come before typology. But this principle is obscured and disregarded, when—as is the general practice—the outlines of phonetic analysis, even the phonetic description of a particular language, are introduced and prepared by some typological survey of "the sounds of human speech." Phonetic analysis, though it requires a general scheme of phonetic dimensions, has no use for a ready-made list of typical elements. It is phonetic *comparison* that requires such a list.[32]

When we undertake to compare different languages, then we must "attempt to develop a typology—a taxonomic frame of reference in terms of which different phonologic systems can be classified and compared."[33] Here, the various dimensions that have been used in the phonetic description of different languages will all be included, and they will be given enough divisions to allow us to place every given element; the mere "axes" of the system will be transformed into something like measuring rods.

At the same time, comparison will require that the languages which are being compared be *re-described* in terms of the general "taxonomic frame."

[32] If we accept an arbitrary restriction of contrast to "binary oppositions," then "the scheme of phonetic dimensions" and "the list of typical elements" come to be one and the same thing. For, in that case, any dimension is just a pair of elements. However, for the ordinary purposes of phonetic analysis, such a scheme does not appear to be the most adequate. In fact, with the acceptance of R. Jakobson's doctrine of "binary opposition," interests in acoustic typology seem to have encroached upon the interests of phonetic analysis.

[33] C. F. Hockett on the purpose and plan of his *Manual of Phonology* (p. 1).

For example, having characterized English vowels in terms of three degrees of opening—high, mid, low—we may now have to characterize them in terms of a scale of four, five, or six degrees, in order to determine their relation to the vowels of possibly different three degrees of opening occurring in another language. In order to mark inter-lingual similarities and differences, we must have a measure which provides a mark for every given element. That it supplies such a measuring tool, is one of the most important contributions of a general phonetic typology.

We can see how analysis and comparison may come to be confused. Precisely because comparison involves its own re-description of different languages, confusion with the original structural descriptions is a constant danger. Comparative or typological description cannot *replace* the original. For the general scheme of dimensions and units which is the most adequate for comparing different languages is not the most adequate for the description of each in its own right. And more than that: the general scheme which is the most adequate for comparing *some* languages, say, English and French, will not be the most adequate for comparing others, say, English and Japanese—a point worth remembering for anyone concerned with the techniques of foreign-language teaching, and hence with very narrowly restricted comparisons.[34]

It is clear, then, that even if we had already arrived at some final phonetic typology, we should not be in a position to shed the techniques of that original analysis which supplied the data for the general typological inventory. These techniques would still be required (a) to bring out clearly the *immanent* phonetic structure of any particular language, and again (b) to permit such limited comparisons as will bring out the *specific* similarities and differences within any narrower group of languages.

IV. Conclusions

To sum up: In the original phonetic analysis of particular languages (as in grammatical analysis), nothing is general or universal except the *method*, i.e. the *questions* we ask. We ask them in the form of applying criteria of relevance, and again in the form of choosing and applying dimensions for the description of the relevant elements. We do not confront the language with a list of possible answers. We do not pick elements from some pre-established inventory; the given material itself, "responding" to our criteria of selection, will fill the chosen dimensions.

[34] Einar Haugen on "bilingual description" in his *Bilingualism in the Americas*, 1956, (= Publication of the American Dialect Society 26), p. 41.

That distributional techniques of analysis require the application of a prejudicial list of elements, besides being extremely complicated in themselves, is a most serious drawback to their employment. By contrast, what is presupposed in the application of semantic criteria—namely, a certain non-analytical acquaintance with the language itself—is only an organic part of our study.

This is not to deny the significance of studies of distribution. The procedure of commutation itself implies distribution-statements: statements (a) of the paradigmatic relations among the contrasting items, and (b) of the syntagmatic relations between them and their environments. But commutation implies also a third kind of relation—namely, that *functional* relation of the contrasting items to any whole sign-unit in which they occur. And it is by taking account of this *part–whole* relation, that the operation of contrastive substitution serves as a test of relevance.

It would appear that criteria of relevance can only operate with part–whole relations: i.e. in phonetics, with relations between sounds or sound-features, on the one hand, and signs, on the other.

*

At this point we have to face some last and truly radical doubts. I refer to the view which, I know, is shared by many, namely, that an argument—*any* argument—of the kind I have tried to present ought to be rejected simply on the ground that it can serve no practical purpose.

A challenge of this kind, which is not concerned with points of detail but aims at the whole drift of the argument, might be stated in some such words as these:

"Even if relevance in phonetics is semantic relevance, and criteria of relevance are semantic criteria, there are powerful reasons—reasons more powerful even than the demand for criteria of relevance—against using such criteria. Far better use substitutes, however imperfect they may be. For, *any* argument in favor of using semantic criteria represents a retrograde step in the development of linguistic studies. If we acted on such an argument, we should only relapse into relying on inarticulate and subjective intuitions of meaning—the very thing from which the study of distribution has been trying to rescue us. Basically, whatever the complications and the inadequacy of our present techniques in distribution analysis, the approach itself must be judged to be on the right lines."

I think this challenge can be met. It seems to be founded on a basic misunderstanding. It assumes that there must be a conflict of criteria, where there is none. But we are not required to choose. Having decided

that we shall use semantic criteria of relevance, we may yet be in full agreement with the fundamental demand for something more articulate and more controllable than bare intuitions of meaning can be; and also in agreement with the view that studies of distribution hold the key. However, the question—and it is the crucial question—is: *The distribution of what*? *Of sounds or of signs*?

What I would suggest is this: The distribution of *phonetic elements*, though worth stating, can contribute nothing towards making us less dependent on intuitions of meaning. The help we may expect from distribution studies (not, indeed, in trying to replace the appeal to meaning, but in the endeavor to make it more articulate, more controllable, and less a matter of mere intuition) will come from exploring the distribution of *signs*, not of sounds. If we wish to go beyond saying that the substitution of *b* for *p* in *pet* corresponds to a substitution of one meaning for another, then we shall have to compare the distributions, not of *p* and *b*, but of *pet* and *bet*: that is, the characteristic syntagmatic collocations and paradigmatic replacements, which will be found to be different for the two words. These distributional relations of the signs, again, will have to be studied as obtaining within the larger unit-functions of whole utterances, and ultimately, of the contexts of situation, in which the utterances "make sense."

This kind of distribution statement would be extremely interesting. But, clearly, so far from replacing semantic criteria, it would succeed in establishing them more securely.

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Hesitation Phenomena in Spontaneous English Speech

[This paper reports an exploratory investigation of hesitation phenomena in spontaneously spoken English. Following a brief review of the literature bearing on such phenomena, a quantitative study of filled and unfilled pauses, repeats, and false starts in the speech of some twelve participants in a conference is described. Analysis in terms of both individual differences and linguistic distribution is made, and some psycholinguistic implications are drawn, particularly as to the nature of encoding units and their relative uncertainty. A distinction between non-chance statistical dependencies and all-or-nothing dependencies in linguistic methodology is made.]

INTRODUCTION

Although references to pause and silence occur frequently in the literature of structural linguistics, very little research of the kind undertaken here has been carried out. A brief examination of three important works will illustrate the customary treatment of pause and hesitation in language description.

In his list of the phonemes of Chicago Standard English, Bloomfield (1933, p. 92)* includes [,], which he describes as being ". . . placed between primary symbols, the pause, often preceded by rising pitch, that promises continuation of the sentence: *John, the older boy, is away at school. . . .*" This is a *pause-pitch* or *suspension-pitch* phoneme which consists of a rise in pitch before a pause within a sentence. A later comment (p. 185) by Bloomfield suggests that the pausal component of this phoneme is a useful, but not a necessary part, of the criterion for its identification.

Harris (1951, p. 174) uses pause in the same general way, as an index of juncture:

> 12.52. Intermittently Present Pause. We may also find in some languages that loose contact and division between breath-groups of phonemes occur sometimes (though not always) at morpheme boundaries, but practically never within a morpheme . . . except as intermittently present features in the utterance. They are free variants,

* All bibliographic references are identified at the end of the article.

and do not occur every time morpheme boundary occurs. But in some occurrences of the morpheme sequence those pauses would constitute observable evidence of morpheme boundary.

In many cases, however, these pauses come at points containing phonemic junctures. At these points in the utterance we find segments which occur only at utterance boundary or at points of intermittently present pause, and which are phonemicized into junctures or into sequences of some phoneme plus juncture. We can then say that the pause (when it occurs) is an occasionally-occurring free variant of the phonemic juncture.

Probably the most explicit and systematic employment of pausal phenomena as an element in linguistic analysis is seen in Bloch's work on Japanese (1946), where he states (p. 201):

1.2. All pauses within a sentence are preceded by level intonation. Every such pause is *faculative:* repeated utterances of the same sentence (by the same speaker or different speakers) will show the pause sometimes present, sometimes absent, without any change in meaning. The presence or absence of pauses within a sentence depends partly on stylistic factors (with more pauses in emphatic or affective speech), partly on the tempo and care of utterance.

However, some facultative pauses are more constant than others, appearing more consistently in repeated instances of the same sentence. It is enough to distinguish two *ranks* of facultative pauses: *higher* (more constant), marked with a semicolon; and *lower* (less constant), marked with a comma.

Bloch also defines a "minimal pause group" which provides one of the criteria for defining words in Japanese. An interesting aspect of Bloch's discussion is the ranking of pauses on the basis of frequency in a given environment and the implication that they are not randomly distributed with respect to linguistic forms.

It seems fair to conclude that pausal phenomena, while they are often mentioned by linguists, function essentially as non-significant events which may serve to identify linguistically relevant units, such as junctures located at the boundaries of phonemes, morphemes, words, phrases, and sentences. The physical character of pause as the term is used by linguists is seldom specified. While it is likely that many of the instances of pause that are referred to in linguistic description overlap with the hesitation types defined below, there is one basic difference between pause and hesitation: hesitations, as we define them, refer to events that are relatively gross and easily observable, while the juncture-pauses of linguistic theory are quite short in duration and much harder to observe and record. In addition, hesitations often interrupt the flow of speech while juncture pauses do not.[1]

[1] This distinction is taken from Lounsbury, in Osgood and Sebeok (1954), p. 98.

Thus, under "hesitation" we also include the phenomena which classical rhetoric called aposiopesis and anacolouthon. To quote Bloomfield again (p. 186):

> Often enough non-linguistic factors interfere with construction; what the speaker has said is nevertheless meaningful, provided he has already uttered a free form. In *aposiopesis* the speaker breaks off or is interrupted: *I though he* ——. In *anacolouthon* he starts over again: *It's high time we—oh, well—I guess it won't matter.* When a speaker hesitates, English and some other languages offer special parenthetic *hesitation-forms*, as [r] or [ɛ] in *Mr.—ah—Sniffen* or *Mr.—what you may call him—Sniffen* or *that—thingamajig—transmitter.*

This statement defines two of the hesitation types described below—*False Start* and *Filled Pause.*

Bloomfield's observation that non-linguistic factors are operating here agrees with our results, and leads us to the psychological side of the question. Although possessing a long tradition of research on verbal behavior, experimental psychologists have usually preferred to rely on normalized written material and have therefore shown little or no interest in the hesitations present in spoken language. Various sorts of speech deviation have, however, been the concern of clinical psychologists, especially psychoanalysts, for many years. Freud has devoted a good deal of attention to the interpretation of speech slips, which include the forgetting of words, the replacement of intended words by others, and transpositions of sounds and words (see, e.g., Freud, 1938). He has offered psychoanalytic explanations of these phenomena, and psychotherapists have often observed that a sudden onset of hesitations, extended silences and speech disturbances in general indicates that an anxiety-provoking area in the patient has been touched.

In a recent series of studies, George Mahl (1956*a*, 1956*b*, 1957)[2] has attempted to specify more exactly the relation between the speech disturbances and level of anxiety in both clinical interviews and stressful role-playing situations. He has established eight "disturbance" categories: (1) "ah"; (2) sentence correction; (3) sentence incompletion; (4) repetition of words; (5) stutter; (6) intruding incoherent sound; (7) tongue slip (neologisms, transpositions, etc.); (8) omission of words or parts of words.[3]

[2] See also G. Mahl (1958).

[3] These equate as follows with our hesitation types: "ah" is equivalent to our Filled Pause; sentence correction equates with our Retraced False Start; sentence incompletion is equivalent to our Non-Retraced False Start; repetition plus stutter corresponds to our Repeat. Categories 6, 7, and 8 have no counterparts in our analysis, and Mahl used nothing strictly equivalent to our Unfilled Pause (although he has studied extended silences).

He has also developed three speech disturbances ratios: *general*, which is the number of disturbances divided by the number of words in an utterance; *ah*, which is the number of instances of category 1 divided by the number of words; and *non-ah*, which is the number of instances of categories 2–8 divided by the total number of words. He finds that the *non-ah* ratio correlates positively with degree of anxiety as judged by a trained observer and the *ah* ratio correlates negatively, whereas the *general* ratio is independent. He presents anecdotal material which indicates that "the vast majority of the speech disturbances are 'unintended' and escape the awareness of both speakers and listeners, in spite of their very frequent occurrence." He also finds "striking individual differences in the frequency of speech disturbances, and in the relative predilections for the individual categories of disturbance." We have preferred to call our measures "hesitations" rather than "disturbances," but the overlap with Mahl's categories is obvious, and our results accord very well with his where direct comparisons are possible.

Another research program bearing closely on our work is that being conducted by Frieda Goldman-Eisler (1954*a*, 1954*b*, 1955, 1957, 1958*a*, 1958*b*). She has studied the rate of speech production, hesitation pauses, speech-breathing and their interrelationships under various conditions, particularly the psychiatric interview. Among her significant observations are the following: (1) Variability in total speech rate proves to be mainly a function of the time spent in hesitation pauses and not in time spent in articulation. (2) Short utterances are much more variable in speech rate than long utterances, a high degree of stability within the individual being found for utterances longer than 100 syllables. (Our samples are mostly 100 *words* or longer, hence well within the range of stable rate.) (3) Interruptions in the flow of speech are occasioned by both breathing pauses and by hesitation pauses; the former seem to vary more with emotionality and the latter more with cognitive processes, e.g., prevocal formulation of utterances, word finding and the like. (4) Most relevant to this study is Goldman-Eisler's report (1958*b*) that hesitation pauses anticipate sudden increases in information or uncertainty in the message being produced (1958*a*, p. 67):

> . . . the close relation found to exist between pauses and information on the one hand and fluency of speech and redundancy on the other, seems to indicate that the interpolation of hesitation pauses in speech is a necessary condition for such an increase. . . . Fluent speech was shown to consist of habitual combinations of words such as were shared by the language community and such as had become more or less automatic. Where a sequence ceased to be a matter of common

conditioning or learning, where a speaker's choice was highly individual and unexpected, on the other hand, speech was hesitant.

Our own findings point to very much the same general conclusion.

Lounsbury has discussed hesitation phenomena along highly similar lines (in Osgood and Sebeok, 1954, pp. 98–101). Three of his hypotheses are worth repeating since our results bear directly on them.

HYPOTHESIS 1: Hesitation pauses correspond to the points of highest statistical uncertainty in the sequencing of units of any given order.

HYPOTHESIS 2: Hesitation pauses and points of high statistical uncertainty correspond to the beginning of units of encoding.

HYPOTHESIS 3: Hesitation pauses and points of high statistical uncertainty frequently do not fall at the points where immediate-constitutent analysis would establish boundaries between higher-order linguistic units or where syntactic junctures or 'facultative pauses' would occur.

Hypotheses 1 and 3 are testable, and we offer some evidence on them in this paper. Hypothesis 2 has an element of circularity, in that no independent method of defining encoding units has been developed. However, the implication of the hypothesis is that it should be possible to identify functional psycholinguistic units of encoding through analysis of hesitation phenomena.

PROCEDURE

This study is based on analysis of a sample of slightly over 50,000 words taken from a conference held at the University of Illinois in 1955. The selection of the sample was non-random in that it included almost all of the relatively longer utterances in the text, those under 80 words being omitted. The final corpus consists of 163 utterances by 13 male speakers, all professional people, with a mean utterance length of 309 words. The data were tape recorded and transcribed in normal English orthography by secretaries who were instructed to produce as literal a transcription as possible. They were told to avoid any "cleaning up" of the text and asked to preserve everything said by each speaker regardless of their opinion as to its grammatical accuracy. This resulted in what might be called a "semi-literal" text, in that it lies somewhere between a rigorous phonemic transcription and the usual normalized orthographic version of spoken English. This compromise arose primarily from the nature of the study. It seemed wise to sacrifice depth for breadth in an exploratory investigation, with the hope that any significant relations that might have been affected by this procedure could be checked in future studies using a fully adequate linguistic transcription.

Four Hesitation Types were defined.

1. REPEATS (R): All repetitions, of any length, that were judged to be non-significant semantically. In the utterence, *I I saw a very very big boy*, *I* is repeated but *very* is not, since in the latter case the repetition intensifies *big* and thus changes the meaning, while this is not the case for *I*. We assume that the utterance with or without the repeated *I* would be judged the "same" in meaning by English speakers, while utterances with one as against two occurrences of *very* would be judged "different." A REPEAT can vary from a single phoneme to an extended stretch that could, theoretically, be of any length but actually does not exceed four or five words in this corpus.

2. FALSE STARTS (FS): All incomplete or self-interrupted utterances. *I saw a very . . .* is an incomplete utterance with FS following *very*, while *I saw a very big // a very small boy* is a self-interrupted utterance with FS following *big*. The second case represents an instance of RETRACED FS and the first an instance of NON-RETRACED FS. This distinction is made on the basis of whether or not the speaker backed up in an attempt to correct one of the words he had already used. Intonational patterns and word order are the major linguistic cues in the identification of this type.

3. FILLED PAUSES (FP): All occurrences of the English hesitation devices [ɛ, æ, r, ə, m]. Of these alternatives [ə] is by far the most frequent in our data.

4. UNFILLED PAUSES (UP): These were marked when there was judged to be an abnormal hesitation in speech that could not be referred to the three previous categories. UP has two major forms: silence of unusual length and non-phonemic lengthening of phonemes. This is necessarily a matter of judgment on the part of listeners (here, the authors) familiar with the pace and style of a particular speaker. What may be clearly noted as an instance of Unfilled Pause for one speaker would not be so judged for another speaker, say, with a slower rate of delivery.

On the whole, these four categories refer to variations in speech that have not been the explicit concern of linguists, although Unfilled Pauses would certainly include many instances of Bloch's facultative pause. False Starts seem to cover much the same ground as is included in the terms aposiopesis and anacolouthon.

A word of caution should be entered concerning the references to intonation, non-phonemic length and other linguistic correlates of the hesitation types. These are after-the-fact interpretations; the actual scoring involved judgments that were not explicitly based on these considerations. The authors, as native speakers of English, attempted to categorize and locate the hesitations that seemed to be present in the text. Scoring was based on

the tape recording with the written text being used as a guide and a framework within which the scores might be placed. Preliminary preparation of the judges was limited to a general discussion of the types and a one-hour session in which a sample text was jointly scored.

After each author had independently scored a semi-literal transcription, a final text was obtained by combining the two ratings and counting only those hesitations where both judges agreed as to type and location. This means that some hesitations which actually occurred were not included, but it also makes it highly probable that all hesitations upon which the analysis is based really did occur at the points indicated. Very high product-moment correlations (r) were found between the independent ratings for utterance totals (R: +94; FS: +98; FP: +99; UP: +88). Specific correlations based on agreements and disagreements at each point presumably would have been lower. These latter, and more accurate, reliability estimates could not be obtained due to the impossibility of stating the number of cases where *neither* judge had indicated a hesitation. This uncertainty led to the conservative decision to consider only instances of complete agreement. In addition to the frequency of each type of hesitation per utterance, the speed in words per minute of each utterance was calculated.

In the following sample taken from the final text, R is indicated by underscoring the items repeated, FS by //, FP by /ª and UP by /.

As far as I know, no one yet has done the // in a way obvious now and interesting problem of / doing a // in a sense a structural frequency study of the alternative / syntactical ///ª in a given language, say, like English, the alternative /ª possible structures, and how // what their hierarchal / probability of occurrence structure is. Now, it seems to me you w-w-will need that kind of data as base line from which to judge or study deviations—in particular style in the clinical situation and so on. If we get this // now in other words, if you find that in the clinic, say in the /ª protocol of a patient, that the distribution of these /ª alternative structures are precisely what they are in ordinary /ª communications, then there's no evidence that this, at least i-is a / relevant variable for the clinical situation. On the other hand, if, knowing that standard sort of thing for English / speakers, /ª then you find you get oscillations about that in different stages of therapy and so on, then it becomes, I think, very very very relevant. For example, a // if under // I was speaking to you, George, the // at supper last night, I think. If /ª you find that /ª at each point, say, in English structure you have an alt- // several alternatives, some of which are highly probable / in ordinary English sentences, syntactical sequences of, for example, /ª adjective, noun, verb /ª vs. a a de-dependent phrase inserted, and so on. But these have / differing probabilities of occurrence in ordinary English. Now, if you find, for example, that in high points of anxiety and stress in the therapy situation that he / shifts into very simple ones—I mean ones that are of a higher // much higher probability, whereas when he's being intellectual, if you will, and / resisting, you tend to get /ª relatively low probability / sequences, you see. (Subject D, Utterance 10.)

RESULTS

LINGUISTIC DISTRIBUTION OF HESITATION TYPES

METHOD OF ANALYSIS. We have made a preliminary attempt to state the distribution of the four hesitation types with respect to English word classes as described by Fries (1952).[4]

Fries classifies English words on the basis of their occurrence in relation to other words rather than on semantic grounds. For example, one class of words can be established by first listing the following three sentence contexts or frames:

Frame A: The ____ is/was good.
Frame B: The ____ remembered the ____.
Frame C: The ____ went there.

We then place in one class all the words which can be inserted in one or more of the blanks and still form a grammatical English sentence. The following sentences illustrate this procedure: The *coffee* was good. The *man* was good. The *clerk* remembered the *tax*. The *husband* remembered the *food*. The *team* went there. The *dog* went there. We would therefore conclude that *coffee*, *man*, *clerk*, *tax*, *husband*, *food*, *team* and *dog* are members of the same class, along with a great many other English words, of course, that could be inserted in these frames. A word such as *the*, however, cannot be placed in these frames and thus must be placed in a different class established by other frames. Each English word class is similarly defined in terms of occurrence within a set of frames.

Obviously, the class described has much in common with the traditional class of nouns defined semantically. For those who may be unfamiliar with Fries' system, we present below the nearest traditional equivalent of his classes. It cannot be too strongly emphasized that the overlap is far from complete, and our analysis depends entirely on the description by Fries and not on the traditional classification. We have also followed Fries in making a distinction between the four major classes (which we call "lexical") and the "function" word classes.

	Fries' Classes	*Traditional Classes*
Lexical Words	1	Nouns
	2	Verbs
	3	Adjectives
	4	Adverbs
Function Words	A	Articles, Possessive Pronouns, Numbers, etc.
	B	Verb Auxiliaries
	C	"not"
	D	Adjective and Adverb Modifiers
	E	Connectives
	F	Prepositions
	J	Words uniting phrases in a single sentence.

[4] The summary of Fries' system which follows is for the benefit of readers who have not been trained in linguistics.

In some of the tables which follow, Fries' Class 1 has been arbitrarily split into 1p, which contains subject personal pronouns (*I, you, he, she, it, we, they*), and 1, which includes the remaining members of this class. In addition, the Function Word classes G, H, K, L, M, N, and O are grouped in one category entitled "other function words" in Table 1.

If we are to describe the distribution of hesitation types we must first locate them relative to the sequences of words in our corpus. The overwhelming majority of Filled and Unfilled Pauses are located at word boundaries and their occurrences can therefore be described as falling generally between words of any class.

". . . you tend to get /ª relatively low probability / sequences . . ." (Subject D, Utterance 10.)

The Filled Pause (/ª) here is located between a class 2 word, *get*, and a class D word, *relatively*, while the Unfilled Pause (/) falls between two class 1 words, *probability* and *sequences*.

Repeats cannot reasonably be located between words, and we have therefore associated each Repeat with the class of the words or parts of words repeated.

". . . a measure of the / of the structure of English, the probabil-probabilistic structure of /ª English language . . ." (Subject D, Utterance 4.)

The first Repeat involves two words, *of* (class F) and *the* (class A), while the second is associated with the class 3 word *probabilistic*. In the scoring of this part of the utterance, classes F, A, and 3 would each be given one association with the Repeat hesitation type.

The most convenient way of describing the distribution of False Starts is in terms of the class of the word immediately preceding the break. We adopt this convention for purposes of comparison with the other types. Most False Starts do occur at word boundaries, but many occur at morpheme boundaries within words and some even within morphemes, as the following examples illustrate:

False Start (//) at word boundary:

". . . And the next one would // might be that the the receiver of the action becomes initiator . . ." (Subject 1, Utterance 3.)

False Start at morpheme boundary within word:

". . . whenever anyone mentions communism-capital // capitalism, communism comes up . . ." (Subject A, Utterance 3.)

False Start within morpheme:

". . . if you were to select your samples differently and to /ª to consider the conte- // the larger context from which you select this 250 word sample . . ." (Subject B, Utterance 7.)

In the last example we concluded that the False Start follows the class 1 word, *context*. We determine that the phonetic sequence [kánteʔ] is class 1 by the presence of the class A word *the* preceding it and the use of the full word *context* in the retracing which follows the False Start. A clear decision as to the class of the words preceding False Starts is often not possible when the break comes early in the word and no retracing follows, for example:

". . . We disregard the structure; we simply take /ª as our unit /ª w- // more or less the whole interview . . ." (Subject E, Utterance 5.)

On the basis of our semi-literal transcription we might guess that the speaker intended *whole* before the False Start, but an examination of the tape shows that the sound preceding the break was the voiced semivowel /w/ rather than the phoneme /h/ which begins *whole*. In cases of this kind no score was recorded.

A second type of ambiguity arises when the phonemic shape of the word preceding the False Start can be recognized but its class affiliation is uncertain due to a lack of following context, e.g.,

". . . I think I have // this is a tool . . ." (Subject 7, Utterance 1.)

The *have* could be either class B (verb auxiliary) or class 2 (verb). In this case one score was given to each of the possible classifications.

FALSE STARTS AND REPEATS. The hesitation types differed distributionally in several respects. Table 1 compares the total distributions of False Starts

TABLE 1. DISTRIBUTION OF FALSE STARTS AND REPEATS BY WORD CLASSES

Fries' Word Class	1p	1	2	3	4	A	B	C	D	E	F	J	Other Function Words	Total
FS	53	200	150	73	42	121	57	10	15	7	54	18	17	817
R	123	67	87	37	6	247	66	7	9	38	172	67	46	972

Probability that difference in distribution of FS and R is due to chance by x^2: < .001.

and Repeats with respect to 11 of Fries' classes. The distributions of the two hesitation types are significantly different from each other by Chi-square test over the 13 divisions of Table 1 at a level beyond .001. Repeats typically involve class 1p words (subject personal pronouns), class A (articles, possessive pronouns, numbers), class F (prepositions), and class J (words uniting phrases); False Starts typically involve class 1 words (nouns), class 2 (verbs), class 3 (adjectives), and class 4 (adverbs). Table 2

TABLE 2. DISTRIBUTION OF FALSE STARTS (FS) AND REPEATS (R) BY LEXICAL VS. FUNCTION WORDS

	FS	R
Lexical Words	518	320
Function Words	299	652
Total	817	972

Probability that differences are due to chance (chi-square test):
FS vs. R $<.001$
FS vs. Chance $<.01$
R vs. Chance $<.001$

presents a direct test of the hypothesis that Repeats and False Starts are reciprocally distributed with respect to lexical vs. function words. The Chi-square value for this test is significant at the .001 level. Using a chance estimate based on the fact that 42% of a random sample of our corpus were function words, we find that False Starts tend to occur with lexical words (.01 level) and Repeats with function words (.001 level) more often than would be expected by chance. One aspect of these distributions warrants special comment: subject personal pronouns (1p, classified by Fries with lexical nouns) appear to function with respect to hesitation phenomena like function words rather than lexical items.

The instances of Retraced False Starts offer some interesting data on speech behavior. Every retracing that involved a correction of the word immediately preceding the break was examined. The word corrected was classified by Fries' system, with only those cases where the class determination was clear being included. The question is this: will the amount of retracing vary with the class of the corrected word, so as to include in the retrace function words just antecedent to corrected lexical items? A two-way distinction was established: retracings involving only the corrected word (I) and retracings involving words preceding the corrected word which were not themselves corrected (II). Given the sentence, *I never saw such a man // woman*, the False Start occurs after the class 1 word, *man*, and the retracing includes only the word corrected (case I). In the sentence, *I never saw such a man // a woman*, the False Start occurs in the same place, but the retracing includes the function word, *a*, which is repeated without correction (case II). Table 3 presents the relevant data. It is clear that there is a statistically significant tendency (.001 level) for retraced corrections of lexical items to include antecedent words (almost always function words), whereas retraced corrections of function words seldom include antecedent items. Two exceptions may be noted: again, subject personal pronouns operate more like function words; and class B words (verb auxiliaries) operate more like lexical items. If we assume that speakers will retrace to a

TABLE 3. FALSE STARTS: AMOUNT OF RETRACING BY WORD CLASSES

Fries' Word Class	I Retracing Includes Only Corrected Words	II Retracing Includes Preceding Words	$\frac{I}{I+II}$
1p	8	4	.667
1	10	39	.204
2	15	34	.306
3	2	12	.143
4	0	4	.000
Lexical Totals	*35*	*93*	*.273*
A	48	7	.873
B	2	13	.133
D	5	0	1.000
F	28	1	.966
Function-Word Totals	*83*	*48*	*.634*

Probability that lexical vs. function-word difference is due to chance (x^2): $< .001$.

boundary of an "encoding unit,"[5] these results suggest one kind of a unit as containing a function word followed by a lexical word.

REPEATS. These tend to involve function words rather than lexical items, as has been shown. We may also ask what classes of words they just precede: do speakers tend to repeat items just antecedent to function words or just antecedent to lexical items? Combining all repeats for speakers A through E, we find that 74% of them just precede a lexical item; this proportion is significantly different from chance, based on the relative frequency of lexical items (58%). It thus appears that Repeats tend to be distributed in a way similar to that of pauses (see below). A final question we ask about Repeats is this: How much of the utterance do they typically include? Again combining the data from our major speakers (A through E), we find that 7% of all repetitions include only a single phoneme, 4% a single syllable, 1% a single morpheme (which is not a word), 71% a single word, and 17% two or more words. It is clear that Repeats characteristically involve the single word, occasionally several words, but only rarely units smaller than the word. This, again, seems to have implications for the nature of encoding units.

HESITATION PAUSES. The first question with which we are concerned is whether or not hesitation pauses of both types are distributed at random in spontaneous speech and, if not, what are the determinants of their distribution. A second question concerns the relation between Filled Pause and Unfilled Pause: are they essentially in free variation, or are their distributions complementary to each other? Rather than attempting to collect

[5] On the problem of "encoding units" see pp. 40f. below; see also Osgood and Sebeok (1954), pp. 50–73, and Sarah C. Gudschinsky, "Native Reactions to Tones and Words in Mazatec," *Word* XIV (1958), 338–345.

and analyze all of the pauses occurring in this large corpus, a sample of sixteen word sequences or frames that occurred relatively frequently in the data was selected for detailed analysis. These sequences would usually be defined as phrases in a linguistic description. Table 4 lists the sequences chosen, according to Fries' classification of the word classes included, and

TABLE 4. DISTRIBUTION OF FILLED (FP) AND UNFILLED PAUSES (UP) OVER SELECTED SEQUENCES

Sequence Type*	Example		Positions 1	2	3	4	5	Totals
A1	/the2house3	FP	98	61	55			214
		UP	50	74	61			185
		total	148	135	116			
F1	/at2home3	FP	37	80	112			229
		UP	44	64	42			150
		total	81	144	154			
B2	/will2go3	FP	42	52	50			144
		UP	18	73	46			137
		total	60	125	96			
A11	/the2manor3 houses4	FP	7	6	5	17		35
		UP	5	13	7	4		29
		total	12	19	12	21		
AA1	/the2three3 houses4	FP	7	3	3	5		18
		UP	4	3	5	3		15
		total	11	6	8	8		
A31	/the2red3 houses4	FP	24	24	19	35		102
		UP	17	36	35	19		107
		total	41	60	54	54		
FA1	/to2these3 houses4	FP	40	32	35	57		164
		UP	50	34	66	33		183
		total	90	66	101	90		
F11	/of2manor3 houses4	FP	3	14	2	16		35
		UP	5	6	4	1		16
		total	8	20	6	17		
F21	/of2going3 home	FP	3	7	1	2		13
		UP	3	7	5	2		17
		total	6	14	6	4		
F31	/across2wide3 streets4	FP	11	35	12	22		80
		UP	7	18	13	6		44
		total	18	53	25	28		
BB2	/may2have3 gone4	FP	7	1	9	10		27
		UP	2	1	14	9		26
		total	9	2	23	19		

* In terms of Fries' classes.

TABLE 4, CONCLUDED

Sequence Type	Example		Positions 1	2	3	4	5	Totals
A331	/the2big3fat4 man5	FP	2	3	3	1	4	13
		UP	1	3	4	7	5	20
		total	3	6	7	8	9	
AD31	/one2very3big4 man5	FP	5	5	0	4	2	16
		UP	1	5	1	7	0	14
		total	6	10	1	11	2	
FA11	/under2the3 manor4house5	FP	4	3	4	4	7	22
		UP	3	4	5	3	2	17
		total	7	7	9	7	9	
FA31	/under2the3 big4house5	FP	14	8	13	11	16	62
		UP	14	16	32	17	7	86
		total	28	24	45	28	23	
F331	/under2heavy3 white4sheets5	FP	1	5	2	3	2	13
		UP	3	2	2	2	3	12
		total	4	7	4	5	5	

presents the frequencies of Filled Pauses (FP), Unfilled Pauses (UP), and their totals occurring in each of the possible inter-word positions, both at the boundaries of these phrases and within them. Illustrative words are given for each sequence. These data are summed over speakers; individual differences in the distribution of pauses have not been analyzed, but casual inspection does not indicate that they are of any magnitude.

Lounsbury's first hypothesis was that hesitation pauses will tend to occur at points of highest uncertainty in spontaneously produced utterances. Since, as Fries notes, there are many more members (alternatives) in his lexical classes than in his function-word classes, we should expect pauses of both types to occur more frequently before lexical words than before function words. Table 5 provides a test of this hypothesis. According to chance, it is assumed, pauses would occur equally in each possible position; actually, both Filled Pauses and Unfilled Pauses are found to occur

TABLE 5. DISTRIBUTION OF FILLED AND UNFILLED PAUSE BY WORD CLASS FOLLOWING PAUSE

	Filled Pause	Unfilled Pause
Lexical Words	418	525
Function Words	360	290
Total	778	815

Probability that differences are due to chance (chi-square tests):
Filled Pause vs. Unfilled Pause < .001
Filled Pause vs. Chance < .02
Unfilled Pause vs. Chance < .001

significantly more frequently before lexical words than before function words (.02 and .001 level, respectively, by Chi-square test). Lounsbury's third hypothesis was that hesitation pauses would often occur at points within phrases where immediate-constituent analysis would not establish syntactical junctures. Inspection of the data in Table 4 shows this to be clearly the case. If we assume that syntactical junctures do not occur within a phrase, then it is obvious that many pauses of both types do not function syntactically. As a matter of fact, approximately half (47%) of all pauses occur within the phrases rather than at their boundaries. Table 6 provides further evidence for the non-chance distribution of hesitation pauses—in this case, for constructions of specific types for which sufficient data for testing were available.

Are Filled and Unfilled Pause in free variation or in complementary distribution with respect to each other? The answer is, clearly, neither. Although both types are about equally likely to occur in any of the phrase sequences studied (*Totals* column, Table 4), they are not equally likely to occur in any of the positions within phrases. Table 5 demonstrates that Filled Pause is relatively more likely to appear before function words, while Unfilled Pause is relatively more likely to appear before lexical words, this difference in distribution being significant at the .001 level by Chi-square test. Table 6 indicates that in particular sequence types,

TABLE 6. CHI-SQUARE TESTS ON DISTRIBUTION OF FILLED AND UNFILLED PAUSES OVER EIGHT SELECTED SEQUENCES

Sequence*	Filled Pause/Chance	Unfilled Pause/Chance	Filled/Unfilled
A1	.001*	.001	.10
F1	.001	.001	.10
B2	.01	.70	.001
A11	.10	.02	.10
A31	.01	.20	.01
FA1	.01	.05	.01
F31	.20	.01	.05
FA31	.05	.50	.001

* In terms of Fries' classes.
Each cell indicates probability that difference is due to chance.

where sufficient data were available for analysis, distributions of Filled and Unfilled Pauses may be significantly different. In phrases of the type *will go* (Fries' B2), we find FP before *will* and UP before *go*; in phrases of the type *the red houses* (Fries' A31), we find FP at the external boundaries and UP within the phrase; in phrases like *to these houses* (Fries' FA1), FP again tends to occur at the external boundaries and UP most often before the lexical items; in F31 phrases like *across wide streets*, the same trend is noted, as is also the case for FA31 phrases like *under the big house*. In other

words, for those constructions that can be analyzed statistically, Filled Pauses occur more frequently at phrase boundaries and Unfilled Pauses at word boundaries within phrases. It should be noted that these are statistically significant tendencies, not cases of absolute complementary distribution in the linguistic sense.

Individual Differences in Hesitation Phenomena

Differences between speakers can be described in terms of both variation between individuals on particular measures and variation within individuals across all measures (e.g., profiles). Table 7 summarizes the data

TABLE 7. SPEED OF SPEAKING (WORDS/MINUTE) AND RATES PER 100 WORDS OF HESITATION TYPES

Speaker*	N of Utterances	Mean Words per Minutes	Mean Rates/100 Words				Totals (R, FS, FP, UP)
			R	FS	FP	UP	
3	4	181	.19	.47	1.88	2.53	5.35
D	38	177	1.39	2.12	2.48	3.58	10.20
8	10	173	1.55	1.16	3.30	1.84	8.77
7	8	163	2.17	1.65	3.10	2.01	9.34
4	5	162	.60	.94	1.54	2.65	6.16
6	3	153	1.70	2.13	2.77	5.74	13.19
A	16	149	1.79	1.91	3.80	3.40	11.64
2	8	144	2.50	1.85	2.96	5.27	13.22
B	22	143	2.21	1.28	6.41	1.80	12.26
1	7	140	3.26	1.34	5.35	2.91	13.61
E	15	136	1.38	1.90	7.21	2.68	13.60
C	21	134	1.45	1.30	3.06	4.51	10.78
5	6	122	1.68	1.18	6.41	4.44	14.50
Grand Means		152	1.68	1.48	3.87	3.34	10.97

R—Repetitions; FS—False Starts; FP—Filled Pauses; UP—Unfilled Pauses.

* Speakers having 15 or more utterances of sufficient length are designed by letters, the others by numbers.

used for these comparisons; speakers are ordered according to their mean rates of speaking in words per minute. The numbers of utterances on which the mean speeds and hesitation types are based varied considerably, as can be seen in the table. We shall limit our discussion mainly to those speakers (A through E) who contributed 15 or more utterances of sufficient length for analysis. It may be noted that for the group as a whole, with a few minor exceptions, Filled and Unfilled Pauses occur more frequently as hesitation types than do Repeats or False Starts.

Significant differences between speakers appear for all of the measures made. SPEED OF TALKING: Only one of the 21 utterances by the slowest speaker (C) is as fast as the fastest speaker's (D) median; reciprocally,

only one of the 38 utterances by the fastest speaker is as slow as the median rate for the slowest speaker. REPETITIONS: Only three of the 15 utterances by the least repetitious speaker (E) exceed the median of the most repetitious speaker (B), and reciprocally two of the 22 utterances by the most repetitious speaker are below the median for the other. FALSE STARTS: Only two of B's utterances have as many false starts as D's median; eight of D's 38 utterances are below B's median, however. FILLED PAUSES: None of D's 38 utterances have as many filled pauses as E's median level; reciprocally, none of E's 15 utterances have as few filled pauses as D's median. UNFILLED PAUSES: None of B's 22 utterances have as many unfilled pauses as C's median rate and, reciprocally, only one of C's 21 utterances shows as few unfilled pauses as B's median. These median tests indicate that, for the five speakers on whom we have sufficient data, differences on these measures are highly characteristic.

Table 8 gives the correlations across all subjects for these measures. The low and non-significant correlations between the hesitation types indicate

TABLE 8. PRODUCT-MOMENT CORRELATIONS OF VARIABLES ACROSS SUBJECTS†

	R	FS	FP	UP	Total Hesitation
Speed	−.121	−.078	−.712**	−.385	−.795**
R		+.027	+.112	+.032	+.181
FS			+.032	+.111	+.148
FP				−.157	+.720**
UP					+.478*

* = .05 > p > .01 ** = .p < .01

† For meaning of abbreviations, see Table 7.

that Repetition, False Start, Filled Pause, and Unfilled Pause are essentially independent variables across subjects. The higher correlations of total hesitations with the two types of pauses than with the other phenomena merely reflects the greater frequencies of the former. The fact that there is a very significant negative correlation between speed of talking and total hesitations might be explained away as simply reflecting the fact that speakers who pause more often must produce fewer words per minute. This proves not to be the case, however—at least not within the range of variation in our speakers. For when we recompute words per minute, counting all hesitations as words (i.e., "ah"-pause equaling a word, etc.), the correlation between original and recomputed speeds of speaking is +.98. Therefore we conclude that fast speakers tend to be "better" speakers in the sense that they make significantly fewer hesitation errors of the types measured here.

It is always possible that correlations based on a group of subjects may wash out important relations within subjects taken individually. As a check on this possibility, rank correlations (*rho*) were computed for the five subjects on whom we had enough utterances to make this procedure

TABLE 9. RANK CORRELATIONS OF VARIABLES WITHIN SUBJECTS*

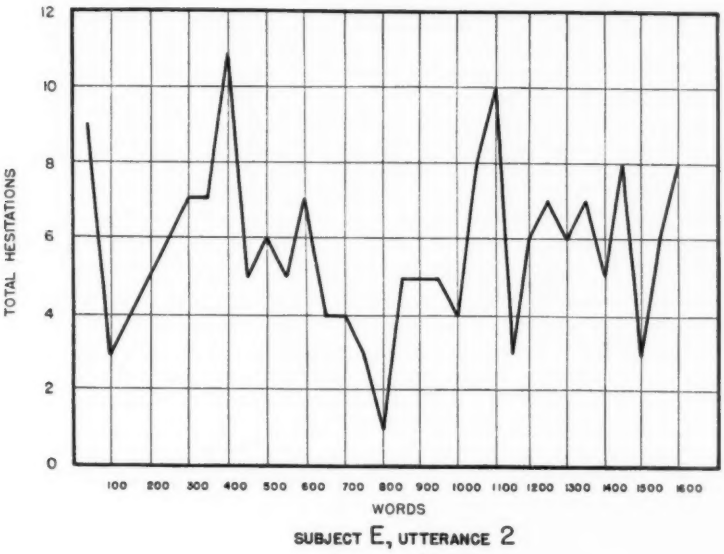
Variables	A	B	C	D	E
Speed/R	+.13	−.11	+.07	+.11	−.09
Speed/FS	+.06	+.39	+.08	+.03	−.01
Speed/FP	−.70	−.59	−.24	−.63	−.54
Speed/UP	−.46	+.17	−.48	−.65	−.52
R/FS	+.36	+.11	−.13	+.09	+.34
R/FP	+.22	+.12	+.07	−.09	−.28
R/UP	−.19	.00	−.17	−.17	+.02
FS/FP	−.12	+.08	−.19	−.21	+.21
FS/UP	−.40	+.15	−.08	−.01	−.56
FP/UP	+.23	−.24	+.09	−.23	−.03
Speed/Total	−.56	−.39	−.40	−.55	−.74

* For meaning of abbreviations, see Table 7.

meaningful. Inspection of Table 9 shows that these variables are related within individuals pretty much as they are across subject averages. The correlations among the hesitation types (R/FS, R/FP, R/UP, FS/FP, FS/UP, and FP/UP) tend to be small in magnitude, different in sign, and averaging toward zero, whereas correlations between speed and pauses of both types (and hence also total errors) are consistently negative as before.

Within these generally consistent relations across speakers there may be uniquely individual tendencies, however. Subjects A and E, for example, show negative correlations of considerable magnitude between False Starts and Unfilled Pauses, as if a sufficient amount of pausing eliminated the conditions for making false starts. Subject B shows a definite tendency to increase False Starts and Unfilled Pauses as his speed of talking increases in utterances, this being in contrast to the other speakers. Furthermore, as Mahl has shown for psychiatric interviews, there may be considerable variation within an extended utterance for a single speaker. Figure 1 plots total hesitations in successive 50-word units for a 1600-word utterance by speaker E; variations from as low as 1/50 to as high as 11/50 are shown, and they seem to follow a non-random course. However, we have not in this study attempted to relate such variations either to the content of utterances or to the states of speakers.

It can also be shown that the relative "preference" for hesitation phenomena of different types varies consistently between individuals. Figure 2 compares profiles for speakers B and D. B's utterances are characterized by a relatively large number of filled pauses and repeats (and



SUBJECT E, UTTERANCE 2

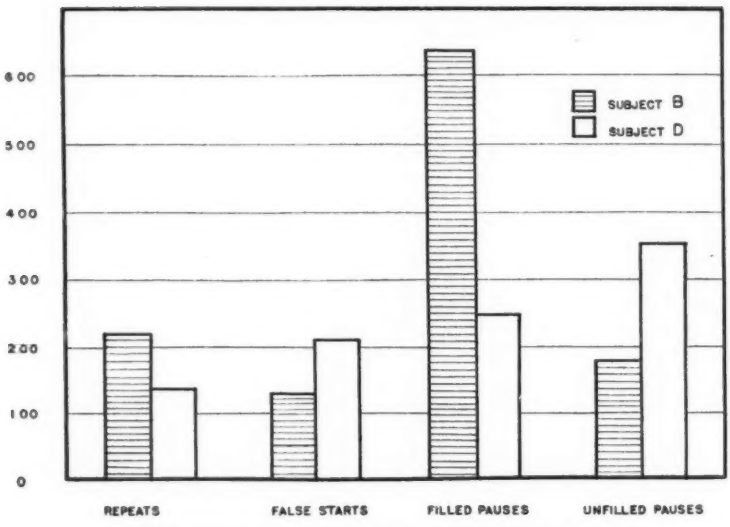
FIGURE 1

a rather slow pace), whereas D's utterances show comparatively more unfilled pauses and false starts (and a rapid pace). Table 10 presents coefficients of concordance (Kendall's *w*) for the rankings of hesitation types by the five major speakers. This statistic, which ranges from zero to 1.00, measures the extent to which the ranking of the hesitation types over his various utterances is consistent within the individual speaker. *W* is significant beyond the .01 level in every case, indicating that individuals—however they may order the types—tend to do it the same way for all of their utterances. Table 10 (like Figure 2) also shows that different speakers

TABLE 10. COEFFICIENTS OF CONCORDANCE (*w*) FROM RANKS ASSIGNED TO HESITATION TYPES

Subject	*w*	Probability that *w* is due to chance	Average ranking of Hesitation Types*			
			1	2	3	4
A	.33	<.01	FP	UP	FS	R
B	.73	<.01	FP	R	UP	FS
C	.64	<.01	UP	FP	R	FS
D	.40	<.01	UP	FP	FS	R
E	.60	<.01	FP	UP	FS	R

* For meaning of abbreviations, see Table 7.



MEAN RATES PER 100 WORDS OF HESITATION TYPES
(SUBJECTS B&D)

FIGURE 2

rank these hesitation types in different ways. In other words, the relative "preference" for hesitation phenomena of different types may be considered an aspect of individual *style* in speaking.

Discussion

Interpretive Summary of Findings

The spontaneous utterances analyzed in this study display consistent differences between speakers, in both the absolute frequencies of False Starts, Repeats, Filled Pauses, and Unfilled Pauses, and in the relative "preference" for these types of hesitation phenomena. However, types of hesitation phenomena are uncorrelated (i.e., independent) both across speakers and across utterances for the same speaker. The negative relations obtained between speed of talking and hesitation phenomena, even when these phenomena are treated as words in counting words per minute, suggest that there are overall differences between individuals in ability to speak spontaneously. The consistency of individual differences demon-

strates that hesitation phenomena may be studied as an attribute of individual style in spontaneous encoding.

Analysis of the linguistic distribution of these hesitation phenomena shows that whereas False Starts typically involve lexical items (blocking after a lexical choice and returning to correct it), Repeats typically involve function words and occur antecedent to lexical items. Repeats thus tend to occur in the same locations as pauses and presumably serve the same function—providing time for selection among diverse lexical alternatives. Repeats are most frequently (71%) of a single word or several words, but rarely (only 12% of cases) of smaller units than the word. When a False Start is retraced, the retracing usually includes the function word or words immediately prior to and associated with that lexical item. Although both Filled Pauses and Unfilled Pauses occur more frequently before lexical than before function words—testifying to the greater uncertainty at these points—and occur within phrases as often as at their boundaries, these two types of pauses are not free variants with respect to positions of occurence. Filled Pauses occur relatively more frequently before function words and at phrase boundaries; Unfilled Pauses, on the other hand, occur more frequently before lexical words and within syntactic phrases. However, these two types of pauses are not in complementary distribution linguistically, since either type *can* occur in any position where the other occurs and does so frequently.

Linguistic and Psycholinguistic Criteria of Structure

Linguists have typically regarded the data of their science as being confined to those aspects of vocal activity that can be precisely described with a finite number of discrete categories. (See Joos, 1950, for a discussion of this point.) According to this view hesitations fall in a non-linguistic (phonetic) area. We may add, however, that they are "outside" language in a way that other phonetic data are not. These latter, while they may be regarded as continuous phenomena, are systematically related to language structure, since continuous variations in phonetic quality may be translated into discrete phonemic categories by means of linguistic discovery methods. Hesitations are not pre-linguistic in this sense; they function as auxiliary events which help to identify and circumscribe linguistic units, rather than as part of the raw data for which a structural statement must account. The fact that they serve this function shows a recognition of their non-random relation to linguistic forms. We may ask whether a structural statement should account for statistically relevant hesitations as it now does for continuously varying phonetic qualities. A definition of hesitations as part of the raw data of a linguistic description would focus attention on

a number of problems concerning the role of statistical methods in a field that has preferred to rely on a discrete 'either–or' approach.

If we define *structure* as a list of elements and statements about the permissible relations among them it may be that the most acceptable compromise will involve reliance upon discrete assumptions to define units (this would exclude hesitations from the list of elements) and continuous statistical assumptions to state their rules of combination and distribution. It is also possible that statistical methods and criteria could be extended to the identification of phonemes and morphemes and this procedure would include hesitations as linguistic units. Yet another resolution would be to consider as linguistically determined only phenomena that are obligatory in a language code, all other non-randomness being considered to be psycholinguistically determined. Linguistic determinants will inevitably control a substantial part of the message—phonetic content, ordering of phonemes and morphemes, and sequences of word classes and phrase types are largely dictated by the rules of grammar. On the other hand, the selection of alternative constructions and of particular lexical items must be explained primarily on non-linguistic grounds. Hesitation phenomena are clearly related to the dynamics of grammatical and lexical selections. Such a classification does not imply that the formal properties of language are independent of psychological processes in language users; to the contrary, obligatory features represent the extreme case of cultural control in behavior.

Implications for the Nature of Psycholinguistic Units

Although the final output of spontaneous speech is necessarily linear or unidimensional through time, this does not mean that the psycholinguistic process underlying it is a simple unit-by-unit sequencing. Carroll (1953) suggested a hierarchical process, in which selection of larger units at "higher" levels is followed by selection of smaller units at "lower" levels, the smaller units filling in the larger constructions. More recently, Chomsky (1957) has challenged the entire notion of the unreeling of speech as a Markov process. Does the study of pausal and other hesitation phenomena provide us with any insight into the nature of psycholinguistic units and their selection?

Depending on which measure we pick, we seem to get a different picture of the functional units of encoding. Repeats characteristically involve whole, single words (suggesting the word as a unit at some level of analysis), yet they usually affect a function word and appear before lexical choices. When False Starts are retraced, it is usually a lexical item that is corrected, but the speaker also includes the closely associated func-

tion words in his retracing. This would suggest that at some level of organization the encoding unit is phrase-like, a lexical core with its tightly bound grammatical context. In at least superficial contradiction to this, however, is the fact that pauses of both types tend to occur just before lexical choices and thus often *between* the function-word context and the lexical core (e.g., *in the / house*). Yet—finally—statistical analysis of the distribution of Filled vs. Unfilled Pauses clearly shows that the former tend to coincide with the syntactical junctures at phrase boundaries, while the latter fall within phrases.

What implications for the nature of the "encoding" (speaking) process can we draw from these data? In the first place, the data as a whole suggest that the speaker is operating with units at least as large as the word, not smaller. But words always involve grammatical as well as lexical selection: e.g. (*walk/-* vs. *walk/s* vs. *walk/ing* etc.) versus (*play/-* vs. *play/s* vs. *play/ing* etc.). We must therefore infer something like a "mixer" just prior to the final common path of output. It is as if we had available at some lower level of encoding a "pool" of heavily practiced, tightly integrated word and phrase units, but selection from this pool requires simultaneous lexical and grammatical determinants. Secondly, the evidence as a whole suggests at least two levels of organization in encoding, which we may call lexical (or semantic) and grammatical (or structural). The best evidence for two such levels or classes of determinants is the fact that the speaker often begins a phrase and then pauses just before the lexical choice within that phrase, as if he had made a structural choice before fully selecting the precise lexical item from the many alternatives available. The statistically significant, if imperfect, complementary distribution between the "ah" and silence pauses also implies some such distinction between levels of selection.

What about the distinction between Filled Pause and Unfilled Pause? We suggest that the main distinction lies in *the duration of the non-speech interval*. Let us assume that the speaker is motivated to keep control of the conversational "ball" until he has achieved some sense of completion. He has learned that unfilled intervals of sufficient length are the points at which he has usually lost this control—someone else has leapt into his gap. Therefore, if he pauses long enough to receive the cue of his own silence, he will produce some kind of signal ([m, ər], or perhaps a repetition of the immediately preceding unit) which says, in effect, "I'm still in control—don't interrupt me!" We would thus expect Filled Pauses and Repeats to occur just before points of highest uncertainty, points where choices are most difficult and complicated. We have also noted that Filled Pauses tend to occur at the junctures of larger syntactical units, presumably where constructional decisions as well as decisions as to *what* to say, content-wise, are

being made. This assumption that "ah"-type pauses are reactions of the speaker to his own prolonged silences at points of difficult decision is consistent with our finding that these two pause-types are merely statistically, not absolutely, different in distribution. A difficult choice *can* occur almost anywhere, although it is more likely at points where both structural and content alternatives are being juggled.

We have the picture, then, of the speaker operating simultaneously on two levels of choices, lexical and grammatical. These are merged in the final selection of word and phrase units that are highly practiced skills, reflecting both types of determinants. The larger the unit being "programmed," the more complex the transformation in Chomsky's sense, or the less probable the sequence, the more prolonged the non-speech interval and hence the greater the tendency for an "ah" or a repetition. Since structural choices typically involve fewer alternatives than lexical choices, the speaker will often initiate a construction before he has completed his lexical decisions—with the result that he may pause slightly in the middle of phrases before such lexical items. Since he is monitoring the sense of his utterances more than their structure, and because errors of sense are more likely than errors of form, he will often halt after a lexical "miss," retrace, and correct it—but the unit of retracing is that of the skill sequence, and this typically includes the function-word context along with the lexical core. Speakers differ in how they manage the complex variables in spontaneous speech. Some slow down to avoid False Starts and syntactical disorder, using "ah's" and repetitions to hold on to the conversational line; others pause (silently) only where lexical difficulties force them to, rather recklessly committing False Starts and only extricating themselves from syntactical impasses by sheer disregard for the rules. This overall picture may be quite wrong, of course, but it seems consistent with the hesitation phenomena we have observed.

Directions for Further Research

Our findings are consistent both with Lounsbury's predictions about where hesitation phenomena should occur and with Goldman-Eisler's reported observations and interpretations. However, the interpretation that such phenomena occur at points of high uncertainty, because they appear more before lexical items than before function words, is rather inferential. Also unsupported is our view that more Filled Pauses and Repeats occur before points of higher uncertainty than do Unfilled Pauses. These matters can be tested by comparing the Thorndike-Lorge frequencies of the lexical items following pauses with a random sample of lexical items following structurally similar points where pauses did *not* occur, and by

comparing the frequencies of lexical items following Unfilled Pauses with those following Filled Pauses and Repeats. We have not yet made the necessary analyses.

It is clear from the work of Mahl that the motivational and emotional states of speakers will influence the frequency of hesitation phenomena and perhaps their distribution. Apart from the interview situation, clinical and otherwise, in which Mahl has worked, it should be possible to study the effects of conversational stress (e.g., implied support or criticism from the audience), of status differences between speaker and listener, of the amount of planning or rehearsal of an utterance, and so forth. These phenomena can also be studied from the point of view of the listener. Presumably the hesitations that slow the speaker at points of uncertainty also permit the listener to catch up, as well as serving to stress the less predictable items. Debate coaches will often encourage their charges to "rough up" their presentations when the season is about half over. It is also possible that natural-appearing pauses and other hesitation phenomena influence the listener's connotative judgment of the speaker, e.g., of the speaker's "sincerity." Similarly, novelists will sometimes introduce pauses (symbolized orthographically by *um*, *er*, etc.) into the speech of a character to convey impressions that are not easily transmitted by the lexical content.

The periodicity of hesitation rate suggested by Figure 1 is of some interest, although we can at present do no more than speculate on the variables at work. Mahl has pointed to anxiety as one source of variation. If anxiety is a tension-producing phenomenon, vocalization is likely to be tension-reducing. This implies that a subject in an anxiety state as he begins to talk will show an initially high rate of hesitations followed by a decreasing rate as speech continues. The level of hesitations may increase again as a result of external factors or as a consequence of the content of his own utterances. An example of this, to which the authors can testify, is the extreme difficulty of talking about hesitation phenomena without having one's own disturbance rate rise rapidly. Changing transitional probabilities during an extended utterance would be another factor. The alternatives available to the speaker are likely to become more and more dispersed as he exhausts the familiar things to be said about any particular topic, and as he moves into areas of low probability words and sequences he will need more time for selection. When he switches topics, he may gain a new statistical foothold. This tendency is probably reinforced by the stylistic requirement of educated English to search for synonyms rather than to repeat previously used words. On the basis of comparison of a few curves like that in Figure 1, we suspect that subjects differ considerably in the periodicity of their hesitation rates, but what the correlates of these

differences may be (e.g., intelligence, personality variables, communication strategy), we do not know.

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ROBBINS BURLING

Language Development of a Garo and English Speaking Child

Toward the end of October, 1954, I arrived with my wife and small son Stephen in the Garo Hills district of Assam, India, where we were to spend most of the next two years making a social anthropological study of the Garo.[1] These people, one of India's tribal populations, number a quarter of a million and speak a language belonging to the Bodo group of Tibeto-Burman.[2] When we arrived, Stephen (who was born on the last day of May, 1953), was one year and four months old and was just beginning to attach meanings consistently to some of the vocal activity that he had been emitting in profusion for many months. His first few words were English, but he immediately came into regular contact with Garo speakers and soon added Garo words to his vocabulary; in fact, for the greater part of the time we were there, his Garo was significantly more fluent than his English. Since there have been few studies of child language in non-Indo-European languages, I kept a record of my son's linguistic development and have assembled the results here.

The study of a child's speech is a delightful one, not the least satisfying aspect being the frequent enjoyment that one's informant shows in having so much attention paid to him by his father. Nevertheless, it is beset with difficulties that never arise in the study of adult language. Children won't repeat themselves the way good adult informants can be persuaded to do. It is next to impossible to try to compare minimal pairs directly, both because of the poverty of the children's vocabulary and because of the difficulty in getting them to repeat forms. Moreover, children speak with less precision and much less consistency than adults. With wide variation

[1] This paper is an expanded and revised version of one read at the 1958 summer meeting of the Linguistic Society of America. The opportunity to work in the Garo Hills was provided by a fellowship from the Board of Overseas Training and Research of the Ford Foundation.

[2] I have assembled a descriptive grammar of the Garo language which is to be published shortly.

within the limits of a phoneme and with the difficulty of comparing minimal pairs it is frequently impossible to specify precisely the time when the child first makes a new contrast within the range of speech sounds that had formerly been a single phoneme. Nevertheless the ideal for which I have striven is an analysis of the successive additions of the distinctive features rather than a detailed description of the ever changing phonetic minutiae of his speech.

Stephen began to use Garo words within a few weeks of our arrival, but his English vocabulary grew steadily and it was several months before Garo became clearly predominant. The eventual triumph of Garo was aided by a protracted hospitalization of his mother which, except for short visits, removed him from close contact with his most important single English model. She was hospitalized through most of March and April, 1955, when he was one year and nine and ten months old. At this time, I spoke to him frequently in Garo, which diluted the effectiveness of the second major English source. Even after this the continued illness of his mother forced him into greater contact with Garos than might otherwise have been the case. The result was steady progress in the Garo language, which I believe he learned in much the same way as any Garo child.

Phonemes at One Year and Four Months

When we arrived in the Garo Hills, Stephen was one year and four months old and had a total vocabulary of a mere dozen words, but even those required a considerable phonemic inventory. He seemed to distinguish three vowel positions; a low unrounded vowel, generally central, though sometimes varying toward the front, as in *papa* 'papa'; a high front unrounded vowel as in *kiki* 'kitty'; and a mid or high mid back and strongly rounded vowel, as in *tu* 'door.'

As these examples also show, he clearly distinguished three stops in roughly the positions of *p*, *t* and *k*. Usually the stops were unaspirated and somewhat varyingly or mildly voiced, and in this case they were used with voiced vowels. Occasionally, however, the stops were strongly aspirated and without voicing, and in such cases they were invariably accompanied by unvoiced vowels, resulting in an explosive whisper: [kʻaʌkʻaʌ] 'custard', [pʻaʌ] 'up'. He interpreted all English voiced stops as unaspirated, but voiceless stops were sometimes interpreted in one way and sometimes in another, although he was generally consistent for any one word. For instance, 'papa' and 'kitty' never had aspiration, while 'custard' and 'up' always did. He also confidently used two nasals, *m* and *n*, as in *ma* 'more' and *nɯna* 'banana'. I never heard the nasals used except with fully voiced

vowels. He had then three vowels, five consonants, and a distinction of voice and aspiration that cut across both the vowels and stops.

At this time all of his words were of the form CV or CVCV; if the latter, the two syllables were always identical. The first progress after our arrival came at one year and five months when he learned suddenly and decisively to use two different syllables in the same word, including either different vowels or different consonants. 'Kitty' was then pronounced as *kiti* rather than as the earlier *kiki* and one of his first Garo words was *babi* (standard Garo *ba-bir-si*) 'cook', his name for our cook.

If this appears to be an extensive phonemic repertory, it should be emphasized that it was available to Stephen at the very threshold of his use of meaningful speech. Though I did not keep careful records of the preceding period, my impression is strong that most if not all of the distinctions which I have described were present in the babbling which preceded his real speech.

Though these phonemes were first used for English words, he soon began using them in Garo words as well. For a considerable time he formed words from both languages with what amounted to a single phonemic system, and so for the first part of the record it is largely possible to ignore the existence of two languages. For the remainder of the paper, rather than adhering to a strictly chronological account, I will discuss in turn the development of various aspects of his speech, and in the final section I will make a few general observations and some comparisons with other children whose speech development has been described.

Consonants

About a month after our arrival, at one year and five months, he began to use a lateral in *lala* 'Emula', the name of his Garo "ayah" (nurse). Soon afterward he began experimenting with affricates and spirants. The affricate was established first in *ci* 'cheese', but by the age of one year and seven months, he had both this affricate and a spirant in *sosa* (Garo *so-si-a*) 'wet.' Both *s* and *c* were pronounced intermediate to the position of the nearest English sounds (*s* and *š*; *ts* and *č*) and from the beginning were very similar to the Garo phonemes.

At about the same time he began to use a labiodental approximating English *f* and with refinement in the voiced/voiceless opposition that was achieved just shortly afterwards, he briefly used *v* as well. Garo does not have phonemes near to this position, and presumably as a result, *f* and *v* failed to become established. In fact they disappeared completely for a period and were replaced in English words by the bilabial stops.

The disappearance of *f* and *v* can be taken as marking his real transition to the Garo language. Everything earlier might be considered to be just as much English as Garo, but only Garo influence can explain the loss of these phonemes. He began to use *f* and *v* once more at two years and four months, but even at 2:7 (years and months), a full year after they first appeared, these consonants were inconsistently articulated.

By 1:7, the rather ephemeral distinction between voiced and unvoiced vowels, and the associated distinction between unaspirated and aspirated consonants, had disappeared, being replaced by a consistent distinction between voiced and unvoiced stops. At that time *p t k* and *b d g* became consistently distinct in the prevocalic position, and the associated vowels were invariably voiced. Stephen's pattern then came considerably closer to both that of Garo and of English, since the languages share the voiced/voiceless contrast. In Garo the contrast is made only for syllable initials, and this is the only position in which Stephen made it for most of our stay.

At just about the same age (1:7), he began to use *w* and *h* both for the English phonemes, and for the Garo phonemes, which are phonetically similar to those in English, as in *hai* which in Garo means 'let's go' and the English word *wet*.

At 1:9, he added *ŋ* to his roster of phonemes. This does not occur initially in either Garo or English and so its appearance had to await the development of more complex syllables than he had had when the other nasals were first used. An example is *doʔoŋ* (Garo *joʔ-oŋ*) 'bug'. This example also shows that at this same age the intervocalic glottal stop appeared. It was several months before he could use the glottal stop in the other positions in which it occurs in Garo.

With the addition of these phonemes, there was only one other Garo consonant outstanding: *j*, a voiced affricate which is intermediate as to position between English *dz* and *J* and is thus the voiced counterpart of the Garo *c*. A *j* first appeared at one year and ten months, but for several months thereafter, Stephen used it only irregularly, frequently substituting *d* both for the *j* phoneme of Garo and for its nearest equivalents in English. As soon as *j* was well established (by two years and three months), Stephen had all of the basic phonemic distinctions of the Garo consonants.

Correct pronunciation involves more than simply keeping the phonemes distinct from each other, however, and further refinement in his consonants consisted of increased precision and vigor in articulation, wider distribution of some consonants, growing sensitivity to allophonic distinctions, and better mastery of clusters.

An example of the way in which he gradually gained precision in his articulation is provided by his velar stops. He had had these since we

arrived in the Garo Hills, but at first it was impossible to describe them more precisely than simply velar stops. Possibly because of the sheer small size of the mouth, but also because of imprecision and variation, one could not hope to specify the exact point of articulation. By one year and three months, however, his *k* and *g* had settled into the far back post-velar position which is characteristic of Garo, and he interpreted English *k* and *g* the same way, giving a distinctively foreign quality to English words with these phonemes.

At 1:6 his syllable pattern was expanded to include the CVC pattern when he was able to use a final *t* in *dut* 'milk'.[3] Final *n* followed at 1:7 in *mun* 'moon', *ŋ* appeared finally (its only position) only at 1:9 and final *m* and *l* appear in my records at 1:10. The *k* sound was used finally by 2:0 but *p* was not regularly used in that position until 2:4. Until then, he substituted either *k* or *t* in place of final *p*, as in *jit* 'jeep', and *cik-a* (Garo *cip-a*) 'shut'. The *s* sound was used finally as early as it was used in any other position and in this Stephen surpassed the Garo pattern, which has final *s* only in a small number of imperfectly assimilated loanwords.

From the beginning his word-final stops were unreleased and therefore corresponded to the Garo allophonic pattern. This was carried over into English so that the final consonants of such words as *bed* were consistently unreleased. Medially he was at first inconsistent. Stops might be aspirated once and unaspirated the next time, so that at 1:10 he pronounced 'papa' either as [pʻapʻa] or as [pʻapa]. At 2:0 he learned to make the consistent distinction between syllable initial and syllable final allophones of the stops which contrast with each other medially, syllable initials being aspirated like word initials, and syllable finals being unaspirated. Garo has no true medial consonants since every one must be associated with either the preceding or following vowel, and the difference can be summarized by specifying the position of syllable juncture. At two years, then, with the establishment of some of the consonantal allophones, syllable juncture was also becoming established.

There is a single phoneme in Garo which if syllable initial is a flap, but if syllable final varies from a lateral much like the English *l* to a sound intermediate between a lateral and a flap, with considerable free variation. At first, Stephen pronounced every instance of this phoneme as [l], which amounted in some cases to the use of the wrong allophone of the correct phoneme. Thus in place of Garo *ra-ma* 'road', he would say [lama]. This

[3] Those acquainted with the Indic languages may recognize some familiar items in my examples. Garo has borrowed heavily from Bengali, including the word for "cow's milk" as opposed to "mother's milk." Stephen learned these as Garo words, which indeed they are, and I do not distinguish between them and older Garo words.

is a substitution which I heard small Garo children make, and in fact Garos recognize it as typical childish speech. At two years and three months, Stephen began to shift his initial examples of this phoneme in the direction of a flap and by 2:5 he could execute an expert flap. However, even at 2:7 he occasionally continued to replace the flap with [l] in words that he had known for a long time. New words were pronounced correctly, but some that had become firmly fixed before he was able to articulate correctly were slow to shift.

At two years and five months, also, he was suddenly able to use the glottal stop in clusters (phonetically these are glottalized consonants, though I find it easier to analyze them as clusters), and also in the phonetically distinct situation of preceding a consonant in the following syllable. He was then able to produce beautifully such Garo words as *naʔŋ-ni* 'your'; *maʔn-a* 'to be able'; and *aʔ-kor* 'hole,' and also said /jʌʔm-bo/ with a glottalized *m* substituting for the *mp* cluster of the English 'jump', together with the Garo imperative suffix *-bo*, so this word expressed the command 'jump!', an example of the phonetic and morphological assimilation of English words into Garo that was typical of his speech.

Garo is not rich in consonant clusters, but my data are not complete even for those which it has. An isolated initial *st* cluster appeared as early as 1:10 in *stas* (English 'stars'), but these were not regularly used for many months. Initial combinations of a stop with *r*, which are the commonest Garo clusters, apparently first began about 2:5. At 2:8 he could make the initial *hw* in English, and could consistently add English plural *s* after other consonants, but other English clusters were still regularly simplified.

The result of this progressive development was a steady approach to the Garo consonantal pattern. He used more consonants pre-vocalically than post-vocalically, which is in accordance with the Garo pattern, and his allophones were settling down to the Garo pattern.

At 2:8 Stephen's English consonantal system still had several lacunae. The single Garo *s* was used in place of the four English phonemes /z, ž, s, š/. Though he eventually learned to make a voiced spirant in imitation, he never did so spontaneously. English *r* was replaced by *w*; post-vocalically it was sometimes omitted all together. He never substituted the phonetically very different Garo *r*. The spirants *θ* and *ð* were replaced by various other phones: *den* 'then', *ti* 'three', *ala* 'other', *samsiŋ* 'something', *bæfwum* 'bathroom'. The labiodentals *f* and *v* were fairly distinct by 2:8. Occasionally they were pronounced as good labiodental fricatives, but more often were bilabial fricatives. Even this latter position kept them distinct from any other phoneme. He did not keep voiced and voiceless consonants distinct in the final position.

Vowels

I have pointed out that at the time of our arrival Stephen had three distinct vowels. At 1:6 several new vowels appeared with great rapidity, and for some time his vowels were in such a state of flux that it was difficult to analyze them systematically. His vowels were always much less well defined than the consonants, and more variable in successive utterances. He sometimes experimented with various vowels, used one briefly and then abandoned it. I once heard him pronounce the word 'moon' with high front rounded and unrounded vowels, a mid central vowel, and a high back rounded vowel in rapid successsion, giving every appearance of groping for a sound that suited him. At 1:6 vowel clusters first appeared with *ai* in *kai* 'Karen'. The two vowels were quite distinct and most easily considered as separate syllables. Other vowel sequences soon appeared: *i* following a front rounded vowel toward the end of the month, and *oi*, *au*, *ui*, and *ia* occurred occasionally at 1:7, all of them being bisyllabic. Most of these were relatively rare at this time, but by then *ai* was well established and frequently used. It is the commonest vowel sequence in Garo. By the end of 1:7 I felt that mid front and back vowels were fairly well established, as in *we* 'wet' and *po* 'Paul', though 'teddy' was still pronounced occasionally as *tidi* rather than as *tedi*.

Various other vowels were heard occasionally. He used a mid central unrounded vowel in [vʌvʌvʌ] for a noise that a dog makes, with the vowel more emphasized than most examples of the mid central English vowel. A mid front rounded vowel was recorded first in conjunction with a following *i*, but later also alone in [tö] 'toy', which was also sometimes [töi]. This even appeared briefly in an unvoiced form in [kʻöʌkʻiʌ] 'cookie'. This and a higher front rounded vowel occurred occasionally through 1:7, but by the end of that month, I was inclined to feel that they were nondistinctive variants of the back rounded vowels. The use of [ʌ] was occasional, but not consistent until later. By the end of 1:7, then, he seemed to be settling into a five-vowel system much like the Garo one.

For some time after this, however, *e* gave him trouble and it was only six months later, when he was just two years old, that it was unambiguously distinct from both *a* and *i*. In fact, for a while the *e* disappeared, and once when he was one year and ten months old, I was unable even to get him to imitate it successfully, though he would imitate many other vowels easily. When trying to imitate *e* he would substitute either [i] or [ai]. The failure of the *e* to become well established until later seems surprising both since *o*, which first appeared at about the same time, was by 1:10 thoroughly established, and because both Garo and English have unrounded vowels

in the mid front position, so there would seem to have been ample chance for him to hear these vowels, and model his own speech upon them.

At 1:10 the mid central [ʌ] was reasonably well established in English words, and by his second birthday, the mid front *e* was again secure. At two years and one month, he seemed to be differentiating a separate and lower vowel from *e* toward the *æ* position of English, although Garo has no comparable vowel. The addition of [ʌ] and *æ* represented his first enduring progress beyond Garo.

By 2:1 Stephen's vowel system was well enough defined and his articulation precise enough to make possible a more conventional synchronic description:

/e/ was a front mid, unrounded vowel. It was close to the correct position of Garo but was used also for the English /e/ of 'bed', which is similar.

/æ/ had become distinct from /e/ and was used in English words only, even though these were generally incorporated into Garo sentences.

/a/ remained a low central vowel, used for both the Garo and the English vowels which are similar to each other.

/o/ was a back rounded vowel, intermediate to the cardinal positions of [o] and [ɔ], and therefore close to the proper position for the Garo vowel. It was also used for English words with similar vowels.

/u/ was settling down to the position of conventional Garo: high, usually central and not quite so sharply rounded as the early high back vowel from which it has been derived. This vowel appeared also in such English words as 'turn'.

/ʌ/ remained rarer than the other vowels. It was phonetically not too different from the non-final allophone of the Garo /i/ (for a discussion of which see just below), but as his speech developed /ʌ/, which was used only in English words, seemed clearly to become separated from /a/ while the non-final allophone of Garo /i/ was derived from the more extreme high front [i]. That is, if there was uncertainty about an English word containing /ʌ/, the uncertainty always was whether or not it was distinct from /a/.

/i/ was a high front and unrounded vowel, something like the English vowel in 'beat' but with no glide. The Garo /i/ has several striking allophones, but for the most part at this time, Stephen used a high front unrounded vowel in all positions.

He used vowel sequences of all sorts easily. He did not tend to diphthongize them, but maintained the syllabic division between them which is appropriate to Garo.

The most serious imperfection in his Garo vowel system at 2:1 was the absence of some of the allophones of /i/. Improvement came at 2:3 when he began to make a consistent allophonic distinction between the /i/ of

open and closed syllables. Non-final Garo /i/ was then pronounced somewhat lower than final /i/ in just about the position of English vowel [ɪ] in 'hit'. This was not yet exactly the proper position of the Garo allophone, which in adult speech is pronounced considerably to the rear of the English [ɪ], but at least some allophonic distinction was being made. The same phone [ɪ] was used in English words such as 'hit' while [i] was used for the English nucleus [iy]. In his speech the contrast between the high [i] and the slightly lower [ɪ] was actually phonemic, since the higher one could be used in English words where it appeared non-finally (such as 'feet'), though this is an impossible position for the similar allophone of the Garo vowel. What in Garo were allophones were actually separate phonemes in Stephen's speech of the time, since he used them in English words, in positions not found in Garo. The next step in the development of his high front vowels was not clearly established until 2:8, when the non-final allophone of the Garo vowel shifted securely to the rear to its proper position [ɨ]. This meant that [ɪ] was used only in English words and [ɨ] only in Garo words. But by this time, as is described below, several other developments had brought about a systematic separation of the vowel systems of English and Garo.

At 2:7 Stephen had still not acquired any further distinctively English vowels. For the complex vowel nuclei of English he either used non-fused sequences of Garo-like vowels, or simple Garo vowels. Thus for /ey/ of 'make' he generally substituted the Garo sequence /e-i/ without the fusing of the English nucleus. Similarly for /ay/ of 'pie' and /aw/ of 'out' he substituted /a-i/ and /a-u/ respectively. For /iy/ of 'heat' he substituted the simple high Garo /i/, for /ow/ of 'boat' the Garo /o/, and for /uw/ of 'food' the Garo /u/.

I have described Stephen as having phonemes that are distinctively Garo ([ʔ]) and others that are distinctively English ([ʌ, æ]), as well as many that were used in words of both languages. At least until early in his third year, however, his speech was most efficiently described as a single phonemic system. It was an approximation to Garo, with the addition of a few phonemic distinctions which are not found in that language. Even at the age of 2:7 his Garo was clearly better than his English, and he could still be described as speaking a variety of Garo that happened to have a great many English loanwords. Some of the phonemes of his speech were found only in these loanwords, but there was no indication that he recognized these words, or these phonemes, as having any special status. Other English phonemes were interpreted by him according to Garo speech patterns.

By 2:8 and 2:9 I felt that Garo and English were becoming differentiated as phonemic systems. Many English vowels now seemed to be becoming

fixed in positions slightly different from the nearest Garo vowels, although since the differences were often small and since there was so much free variation in his speech, it was not easy to specify exactly the moment of separation.

At this time [ɪ] was used exclusively for the English phoneme and the non-final allophone of the Garo high front vowel had moved away to the rear. English /e/ also seemed to be consistently pronounced slightly lower than the Garo /e/ and English /ə/ of 'dog' was now lower than Garo /o/. The high back vowels seemed less stable but again English /u/ of 'put' was at least sometimes pronounced further to the rear than the high mid Garo /u/. I could detect no distinction between Garo and English /a/, which are virtually identical in adult speech.

Most of the complex nuclei were becoming more thoroughly united as diphthongs in the manner appropriate to English rather than to Garo. This was true of /ey, aw, ay, ɔy, ɔw/ and /uw/. For /iy/ he continued to use the high front Garo /i/ which lacked any glide.

In other words, in the two months from 2:7 to 2:9 a systematic separation of the two vowel systems took place, and it was impossible to continue to describe them as one system. This is in contrast to the consonants, where—except for the addition of *f* and *v*—Stephen never went beyond the Garo system, and for as long as we were in India, he simply used the closest available Garo phones as replacements for the English phonemes.

Suprasegmentals

Garo, although a Tibeto-Burman language, does not have phonemic tone, and so the history of Stephen's speech cannot include the development of recognized tones. However, he fleetingly used a distinction which, even if idiosyncratic, was based partly upon tone. At the age of 1:6, I noticed that he made a fairly clear and consistent distinction between the words 'papa' and 'bye-bye' on the basis of length and tone. Both had unaspirated consonants and voiced vowels, but the word 'papa' was pronounced with a higher tone and very rapidly, while the word 'bye-bye' was pronounced with a lower tone and more slowly. Some other words seemed to have similar characteristics, though none were as clear as this pair, and the distinction did not last for long.

A month later at 1:7, length was clearly used as a distinctive feature of certain words which represented noises, such as [di:di::] to represent the noise of an automobile horn, and [ʔoʔoʔo::] for the sound of a crowing rooster. (This rather marginal use of the glottal stop actually preceded its use in non-onomatopoetic words by at least a month.)

DEVELOPMENT OF LANGUAGE SKILLS BY STEPHEN BURLING

Age (Years and Months)	*Consonants*	*Vowels*	*Other Linguistic Progress*	*Important Non-Linguistic Events*
1:4	p t k m n	i u a		Arrival in Garo Hills
1:5	l			
1:6	c	Vowels rapidly shifting		
1:7	s w h (f) (v) voiced/voiceless contrast	i u (e) o a		
1:9	ŋ intervocalic ʔ			Mother in hospital
1:10	(j)	ʌ		" " "
1:11			First unambiguous morpheme substitution	
2:0		ɛ	Syllable and word juncture in Garo	
2:1		æ		
2:2			Recognizes the existence of two languages	Living away from Garo Hills, but contact with Garo continues
2:3	j [r] allophone of l	ɪ		Return to Garo Hills
2:5	Clusters with glottal stops			
2:6			English developing rapidly	
2:8		Systematic differentiation of Garo and English vowel systems		

At 1:7 he used the exaggeratedly high and then falling intonation which is used by Garos to emphasize great distance. Even by two years, however,

his speech was not connected enough to do more than begin to catch incipient intonational patterns. At two years he sometimes used the slightly rising intonation which terminates Garo words, and I heard the typical low and falling English intonation with the word 'there' to convey the sense of 'there, that's done'. Stress does not have an important role in Garo, and I heard no significant stress variations, except for the emphasis of shouting.

Morphology and Syntax

At one year and seven months, I began to notice a few utterances which, from an adult point of view consisted of more than one morpheme. He would say *cabo* (< Garo, *caʔ-*'eat' and *-bo*, the imperative suffix); *galaha* (< Garo *gar-* 'fall' and *-a-ha* 'past'); *kukigisa* (Garo *cookie ge-sa*) 'one cookie'; *guboi* 'good boy'. At 1:10 he was occasionally attaching the form *-gipa* to strings of speech or to nonsense, but only occasionally in positions where this suffix (which makes modifiers out of verbs) could have made any sense. I also recorded different inflectional forms of the Garo first person singular pronoun, *aŋa aŋasa*, and *aŋnisa*, which have somewhat diverse meanings, though presumably he learned these individually and did not construct them himself. His closest approach to substitutability was in phrases with the word *ba-o* 'where' as in *po bao* 'where's Paul?'. I heard this word used once or twice with nouns other than 'Paul', but was still doubtful that these were formed by him. It seemed more likely that all were learned as entire phrases. His speech could thus not be described as having utterances of more than one morpheme, since the constituents were not substitutable. Gradually he acquired more such forms, particularly combinations of verb bases with one or another of the principal verb suffixes which indicate such things as tense. He regularly used the suffix which he heard most often with the verb, as the imperative forms of 'eat' and 'get dressed' and the past of 'fall', since 'falling' was most often discussed after it happened.

Then on the first day of May, at one year and 11 months, I was sure for the first time that he could use suffixes freely and add any one of several suffixes to any verb. This was a decisive step and after that, he constantly produced new forms. On virtually the same day, he also was able to form syntactical constructions with a noun subject and a verb. Both morphological and syntactical constructions came at the same time and once the initial ability to make substitutions was gained, he went forward rapidly with more and more complex constructions.

From his first use of the constructions of verb with a principal suffix, he used the suffixes *-gen* 'future', *-bo* 'imperative', *-a-ha* 'past', and *-a* 'present or habitual', which are probably the ones most commonly used by adults. Shortly there followed *-gi-nok* 'immediate' or 'intentional future' and *-jok* 'recent past', but even a month later at two years these were not used so readily as the others.

Soon after his second birthday he was beginning to use some of the adverbial affixes, which form a characteristic feature of Garo. These affixes are placed directly after the verb base but before the principal verb suffix, and modify the meaning of the verb base in various ways. Early in his third year it was difficult to know how freely he was using these, but the ability to form new constructions with them seemed to be beginning. By then I had recorded the following: *-ja-* 'negative', *-eŋ-* 'continuous', *-at-* 'causative', *-ba-* 'progression toward the speaker', *-aŋ-* 'progression away from the speaker', and *-be-* 'very'.

Shortly after his second birthday I had also recorded a few constructions with another class of verb suffixes, a class which follows the principal verb suffixes, and which can be called terminators. He did not use these frequently but I recorded *-ma* 'neutral interrogative', *-mo* 'interrogative anticipating agreement', and *-de* 'emphatic'.

He also used a few noun suffixes, though none of them as freely as the principal verb suffixes. The most frequently used was *-ci* 'in the direction of', while its more emphatic and precise near synonym *-ci-na* was somewhat less frequent. At various times *-ba* 'also', *-ni* 'possessive', and *-na* 'dative' were used, most commonly with the pronoun for 'I'.

The third and last inflected class of Garo is that of numerals, which are formed from numeral classifiers and the quantifiers. Just before his second birthday Stephen began to experiment with combinations of classifiers and quantifiers, putting them together in the correct way, but with no apparent understanding of their meaning. A few days after his birthday, however, he saw a small bug, and said *joʔoŋmaŋsa* 'one bug', including quite correctly the classifier used for all animals, *maŋ-*. A few days later I recorded him saying *gegini* in referring to two pieces of a cookie, correctly joining the classifier *ge-* to the word for 'two', *-gin-i*.

The only English morphological construction which he had even incipiently used was the plural, which I recorded in isolated instances such as *ais* 'eyes' and *stas* 'stars'. But the plural marker was not used freely and could not be substituted. Although this English plural marker appeared very shortly after he first started using Garo affixes, it had not become substitutable or elaborated the way the Garo affixes had. At the same time he gradually acquired a few English phrases such as 'get down', 'come in',

and 'good night', all of which were of course learned as units, and their parts were not substitutable in any way.

Stephen's first freely formed constructions involving more than one word were those which combined a noun as a subject and a verb as a predicate. The construction in Garo is much like that in English, but his first sentences were clearly Garo. For one thing, they almost at once included a Garo principal suffix on the verb, so the sentences included three morphemes. Examples are: *ba-bi on-a-ha* 'Babi (the cook) gave (it to me)' and *doʔo bilaŋaha* 'bird flew away'. He continued often to use verbs alone without a subject, but since this conforms to adult Garo, where the subject need not be expressed if no ambiguity arises, this was not a babyish trait on his part.

Soon after his use of subject-verb sentences, he started to use verbs with nouns in other cases. My first recorded example is on May 17, two and one half weeks after his first construction: *papaci reaŋna* 'go to Papa'. On June 4 I recorded two three-word sentences (four and five morphemes respectively): *aŋa tolet naŋa*, 'I need toilet', and *lala bi taleŋa* 'Emula is preparing the bed'. In both these examples one word was derived from English, though the word order, morphology and phonology were all completely Garo. This assimilation of English words into Garo was characteristic of his speech for as long as we were in the Garo Hills.

By 2:0 he was also using possessives correctly, attaching the possessive suffix to one noun, most frequently *aŋa* 'I', and then following it by a name of the thing possessed. At that time also there were the first constructions of a noun with a numeral, examples of which have already been given. By early June, then, just after his second birthday and a bit over a month after he first clearly used morpheme substitution, he had used the following constructions (those marked with * were used most freely and frequently, while those in parenthesis were only incipient, and those left unmarked were well established, though not among the commonest):

Morphological constructions
- verb base plus adverbial affix
- * verb base (with or without an adverbial affix) plus principal verb suffix
- complete verb plus terminator
- noun plus principal noun (case) suffix
- (numeral classifier plus quantifier to form a numeral)

Syntactical constructions
- * noun (in various cases) plus verb to form sentence
- * possessive noun plus noun
- (noun plus numeral)

Even at this early stage I had never noticed him using obviously incorrect morpheme order. He spoke in a simplified way, of course, and sometimes left things out, but he did not reverse the proper position of morphemes or words. This is perhaps not remarkable since given the freedom of word order (not morpheme order within the words), incorrect word order is not easy to achieve in Garo. In learning these constructions, he apparently always used the construction first in certain specific examples which he learned as a whole and by rote. After using several of these for a while, he would learn to generalize on the construction, and to substitute different morphemes or words in the same construction.

For the next few months, until 2:3, his grammatical development was largely a matter of consolidating and improving the steps that had already been begun in May at 1:11. By September he was freely and correctly using many adverbial affixes and principal verb suffixes. Both these classes had been considerably expanded. At 2:1 I recorded his use of the general suffix *-kon* 'probably', and by 2:2 he was using it in such a way as to show that he understood its meaning. At 2:2, I recorded the suffix *-ode* 'if', but could not tell whether he understood its meaning or could use it freely. These innovations are significant in that they involve members of still another class of suffixes that had not been represented earlier and which are inserted after the principal suffix but before the terminator if there is one. He also used several of the principal noun suffixes (case markers) correctly and readily. Of the noun suffixes, the possessive marker continued to be used most regularly. His syntax included the simple form of the Garo sentence with nouns in various cases, followed by the verb. However, he sometimes put one noun, often a pronoun, after the verb, which is a correct Garo way of giving emphasis to the noun. More elaborate constructions, with phrases, clauses, conjunctions, and the like, were still absent from his speech.

The only English construction which he could freely use at 2:3 was that with 'more', such as *mo ci* 'more water'. Here *ci* is the Garo for water, but the construction is quite foreign to Garo, and is clearly English. The few other English phrases which he used, including even such complex ones as 'what are you doing', had been memorized as units and the parts were not separable.

By the age of two and a half, he had the system of verb inflection essentially complete, including the use of at least some of the members of every class of verb suffix, and all of the common ones. He used many adverbial affixes easily. I heard examples of as many as three adverbial affixes in one word, which is as many as adults commonly use, though it is not a formal limit. He had also begun to use members of a group of verb

suffixes which can convert verbs to a substantive function. At 2:6 he was able to use the important verb suffix *-gi-pa*, by which Garo verbs are put into a form which can be used to modify nouns. He had used *-gi-pa* much earlier, appending it to strings of nonsense, but such "babbling" had stopped by 2:1, and now the suffix was used correctly and with understanding. A few weeks after this appeared, he added the somewhat similar suffix *-a-ni*, which is used to make abstracts from verbs, such as 'heat' from 'hot' or 'walking' from 'walk'.

He also used members of every class of noun suffix, though he did not use every single suffix. He used most of the principal noun suffixes and a good many of the secondary noun suffixes which follow the principal suffixes, and he frequently and easily used the terminating noun suffixes *-san* 'only', *-dake* 'like, in the manner of', *-ba* 'also', and *-de* 'indeed'. Personal pronouns, which in Garo act grammatically like nouns, were added slowly. He had used *aŋa* 'I' since 1:11, but *naʔa* 'you sg.' was recorded only at 2:4, *ciŋ-a* 'we exclusive' at 2:5, and *bi-a* 'he, she' at 2:6. Others were even later.

At 2:6 he could use numbers incompletely and tried to count, but frequently got the order reversed. Two months later he was able to count to ten with occasional mistakes in Garo, and more consistently correctly in English, and he understood the numbers at least to three. He used several numeral classifiers in Garo quite readily, though except for *-sak* (for people) and *-maŋ* (for animals) it was doubtful whether he really understood their meaning.

He was weaker in the use of particles. He used few adverbial particles except *bak-bak* 'quickly', and only a few of the conjunctions. Lacking most of the conjunctions, he was able to form few complex sentences, though by 2:6 he was making the balanced type of Garo sentence in which two similar verbs, one generally positive and the other negative, are paired together with their associated nouns, without an intervening conjunction.

Reduplication is used grammatically in a number of ways in Garo, but by this time I had not yet detected Stephen using any reduplicated forms. The slow acquisition would seem to indicate that the grammatical reduplication of adult speech has little to do with the reduplicated forms of infantile speech which occur so ubiquitously and which Stephen had used from the beginning of his speech.

Semantics

Besides making steady progress in phonology and grammar, Stephen spoke about a continually widening range of subjects. In some cases he

learned a new word in either Garo or English, and for some time used that word in all circumstances, without ever using an equivalent in the other language. In other cases, however, he simultaneously learned English and Garo words with approximately the same meanings, as though once his understanding reached the point of being able to grasp a concept he was able to use the appropriate words in both languages. I first recorded the English word *swallow* and the Garo *min-ok-a* 'swallow' on the same day. He learned to count in the two languages in parallel fashion, and could recite numbers in either one and keep the two sets generally distinct even before he became confident and consistent in reciting them in the right order. It was clear that in both languages he could recite numbers before he had any real ability to count or to understand the meaning.

By one year and ten months he was beginning to use color terms in both English and Garo, with great interest, but with little understanding. He would point to something and mention a color term in either language that had no relationship to the color of the object. When later at 2:9 he suddenly grasped the meaning of color terms, and was able to consistently call a red thing red, he was able to do so in both English and in Garo simultaneously. At about the same time he started to use various indicators of time, such as *last night*, *yesterday morning*, and corresponding Garo terms, again with little understanding except that he kept terms for past time separated from those for the future. This imprecise use of time markers was in striking contrast to his correct use of spatial terms such as *here*, *there*, and various Garo equivalents, which he had started to use correctly much earlier.

At 2:9 Stephen began for the first time to ask explicitly about words. He would occasionally start to speak, pause, point to something, and ask *wats dis*; receiving an answer, he would proceed to use the word in his sentence. He asked such questions equally well in both English and Garo. He also talked about words, though it was difficult to know how well he grasped the sense of the sentences he used. For instance, he would point to his nose and say *in English it is nose*, or *in Gawo it is giŋ-tiŋ*, but he would sometimes get these reversed and call a Garo word English or vice versa. When he was two years and ten months old, I was able to interest him in playing a game with me. I would ask him a question such as "What does hand mean", and he would promptly supply the Garo equivalent; or, if I supplied him with a Garo word, he would return the English. He gave no indication that he noticed which language I presented him with, but consistently gave the opposite one. When I asked for the meaning of the word "table" however, for which the Garos use a word borrowed from English which is phonetically similar to the original, Stephen paused only

briefly and then supplied the translation "dining room table". At 2:9 also, he started using the word *said* and its Garo equivalent *a-gan-a* correctly and with understanding in direct speech—another indication of his growing awareness of speech as a phenomenon that can be talked about. In either Garo or English he was able to say such things as *I said "sit down!"*.

Bilingualism

As early as the age of 1:6, not quite two months after our arrival, I recorded what then seemed to me like translation. If we asked him if he wanted milk, he would unhesitatingly indicate the affirmative with the Garo equivalent *dut* 'milk'. However, six months later I could still find no evidence that he was really aware of the existence of two different languages in his environment. As in the case of *milk* and *dut* he did recognize that there were two words for certain things and sometimes he would say them together, one after the other, clearly recognizing that they were equivalents. But all his constructions at that time were completely Garo, the only significant English influence being lexical.

We spent about two months from the middle of July to the middle of September (2:1 to 2:3) in Gauhati, Assam, away from the Garo Hills, where Stephen came into contact with several English speakers, as well as numerous Assamese speakers. He continued to be cared for to a great extent by his Garo "ayah," who even sought out the companionship of other Garos, so his contact with that language was never broken. But while we were in Gauhati, I began to feel that he really recognized the existence of the separate languages. He quickly learned who did not speak Garo and rarely attempted to speak to them. He was always more shy with them than with Garo speakers. By 2:3 he could understand a considerable amount of English, but spoke little. He then showed a facility for translating exactly what was said in English into idiomatic Garo. If I asked him a question in English, he was likely to give an immediate and unhesitating reply in Garo.

After returning to the Garo Hills, life was somewhat more normal. He had more continuous and intimate contact with his mother than he had had for some months. This finally resulted at the end of 1955, when he was two and one half years old, in an explosive expansion of his ability in English. Though his English never caught up with his Garo as long as we stayed in the Garo Hills, he developed a taste for speaking English with native English speakers and to my chagrin he came to prefer to speak English with me. Occasionally, when I failed to understand his still very foreign English, he would with obvious condescension translate into Garo, which,

being said more correctly, was easier to understand. He translated without hesitation and with no apparent difficulty in switching from one language to the other. He frequently spoke to me in idiomatic English and immediately repeated it in just as idiomatic Garo for someone else's benefit.

We remained in the Garo Hills until October, 1956, but the pressure of other work prevented me from keeping detailed records of Stephen's speech after April (2:10), while his increasing tendency to speak English with me and the increasing skill with which he spoke in both languages made it both more difficult and less rewarding to do so. When we left the district, there was still no doubt that Garo was his first language (when he spoke in his sleep, it was in Garo) but English had become a flexible means of expression as well. We spent about a month traveling across India. At first he attempted to speak Garo with every Indian he met, but by the end of the month was learning that this was futile. I tried to speak Garo with him from time to time, but he rarely used full Garo sentences even then.

The last time he ever used an extensive amount of Garo was on the plane leaving Bombay. He sat next to a Malayan youth who was racially of a generalized southern mongoloid type, so similar to many Garos that he could easily have passed for one. Stephen apparently took him for a Garo, recognizing the difference between him and the Indians that had failed to understand his language in the past weeks. A torrent of Garo tumbled forth as if all the pent-up speech of those weeks had been suddenly let loose. I was never again able to persuade him to use more than a sentence or two at a time. For a couple of months he would respond to Garo when I spoke to him, but he refused to use more than an occasional word. After this, he began failing even to understand my speech, though it was frequently difficult to know just how much was really lack of understanding and how much was deliberate refusal to cooperate. Certainly at times he would inadvertently give some sign that he understood more than he meant to, but increasingly he seemed genuinely not to understand, and within six months of our departure, he was even having trouble with the simplest Garo words, such as those for the body parts, which he had known so intimately.

Conclusions

In the preceding sections I have described the manner in which one child acquired speech. A few general observations about his linguistic development and a comparison of it with that of some of the other children whose language learning has been previously described remain to be made.

The earliest stage recorded here, with three stop positions, three widely

spaced vowels, and two nasals, corresponds closely to the pattern which Jakobson suggests to be characteristic of an early stage of speech, except that to these Stephen added temporarily the cross-cutting distinction of aspiration in the consonants and unvoicing of the vowels.[4] Stephen was not slower to articulate velar stops than the others, unlike some children that have been studied.[5] Some advances, notably the acquisition of the voiced/voiceless contrast, were systematic and added several new phonemes together, though even this was not carried so far as to add a voiced spirant, which Garo lacks, or (until later) a voiced affricate, which Garo does have. Other phonemes seemed to be added individually and it is difficult to see any consistent pattern in their order of appearance.

Viewing his speech as developing by the successive addition of distinctions, or minimal contrasts, provides for the most part an efficient means for analyzing his progress. However, progress was not without its interruptions. Some new steps, such as the acquisition of the contrast between voiced and unvoiced stops or the ability to make glottalized consonants, appeared so rapidly as to be almost datable to the day. In most other cases, however, months elapsed between his first tentative efforts to use a new sound, and its final unambiguous incorporation into his phonemic system. This meant that his language was progressing on several fronts simultaneously and later developments were often beginning before earlier ones were completed.

Apparently Stephen developed phonemic contrasts more rapidly than some children in comparison with the speed of expansion of his vocabulary. Velten, for instance, notes the large number of homonyms in his daughter's speech which appeared as her vocabulary outstripped her ability to produce phonemic contrasts adequate to keep all the items distinct from one another.[6] This was never the case with Stephen, and from a linguist's point of view the problem was more often finding minimal pairs for comparison. Homonyms arising from failure to make the proper contrasts were not common.

It was notable that in the earliest stages of his speech, he briefly used several distinctions (voiceless vs. voiced vowels, tone, length) that are found in neither of the languages that he was learning. One presumes that if these features had been present in his environment he would have

[4] Roman Jakobson and Morris Halle, *Fundamentals of Language* (The Hague, 1956), pp. 37–42.

[5] Werner F. Leopold, *Speech Development of a Bilingual Child* (Evanston, Illinois, 1939–1949), vol. 2, p. 417.

[6] H. V. Velten, "The Growth of Phonemic and Lexical Patterns in Infant Speech," *Language* XIX (1943), p. 287.

started learning them from that time. It might be suggested then that learning a language is not only a matter of learning not to make certain phones, but also of learning not to make certain contrasts which arise apparently spontaneously as a child first tries to speak.

Two and a half years seems remarkably early to have acquired all of the phonemes and all of the important allophones of the language. However, the fact that at the age of four years and four months, almost a year after returning to the United States, he had not yet completed his English phonemic inventory (/θ/ and /ð/ were inconsistently articulated) would seem to deny any extraordinary precociousness, although in general he did seem to be an early talker. Perhaps this provides some objective measure of one's intuitive feeling that Garo does have a simpler phonemic system than English.

Most studies of child language have put heavy stress on the number of words used in a sentence. This is true of much of the psychological literature, but it is also true of Leopold's work. My feeling as I observed Stephen's language, and my conclusion now, is that the number of words or morphemes is perhaps the least important criterion of grammatical progress. What from an adult point of view are multi-morphemic words, or multi-word sentences, were used before their complex nature was recognized by Stephen. The most significant single advance in his ability came when he learned to make substitutions, and once this was achieved, he was soon able to make sentences with not just two morphemes but with three and more. One simply cannot reasonably speak of a two-morpheme stage of his speech development. (Since Garo has such complex word formation, it is more significant to consider the number of morphemes in a sentence than the number of words.)

It has generally been assumed that syntactical constructions precede morphological ones. Leopold states baldly, "In the field of grammar, syntax comes before morphology. The student of child language becomes very conscious of the fact that the morphological devices are a luxury of fully developed languages. The small child gets along quite well without them, for a short or long time."[7] It is difficult and perhaps arbitrary in many languages to draw the line between morphology and syntax, but it is extremely convenient to make such a distinction for Garo, since there are stretches of several syllables set off by characteristic junctures which can be called words, and the grammatical devices used to form these words are very different from those used to join words together. If the distinction is made, Stephen defied the generalization that syntax comes first by learning

[7] Werner F. Leopold, "Patterning in Children's Language Learning," *Language Learning* V (1953–1954), p. 10.

to make both types of constructions simultaneously. Some reasons for this are obvious: What I am calling morphology in Garo is much more essential than the morphological processes of English, or even of the other European languages. Morphology is not a "luxury" of the fully developed language. Moreover, it is far more regular, and therefore no doubt easier to learn than the morphology of European languages. What I am calling Garo morphology, then, has somewhat the character of syntax in the European languages, but Stephen's method of learning this morphology was not comparable to that by which other children have been observed to learn English syntax. Most histories of child speech have reported a period in which many words are used together with minimal regard for the standard order. Children have been described as using ill-connected sequences of words, and only gradually learning to join them closely together with proper order, juncture and intonational pattern.

One of the striking features of Stephen's speech was the rarity with which he reversed the order of morphemes or words. This was true of his earliest Garo sentences and also was true of his English when it finally began. This was in marked contrast to my own daughter who learned English as her first language two years later, and to other children such as Velten's daughter.[8] My daughter regularly produced such garbled masterpieces as *reading a mommy a book Nono* 'Mommy is reading a book to Nono (her nickname)' or *Nono falling Nono jammies pant* 'Nono's pajama pants are falling down'. Stephen never distorted normal order in this way. I do not know whether this is due to peculiarities of the Garo language or to Stephen. It is true that word order is freer in Garo than in English, but he was consistent in phrases where word order is not optional (such as that formed from a possessor and possessed noun) and he was entirely consistent from the very beginning in the order of morphemes within words, which is as rigid in Garo as word order in English, even though these morphological constructions were as early as syntactical constructions. I could readily observe the gradual acquisition of new grammatical constructions and repeatedly the same pattern was followed. He would first learn a number of examples of the construction by rote and at this stage it was generally difficult to tell whether he understood the meaning of the construction or not, though for the most part this seemed unlikely. He would then generalize the construction and learn to substitute other appropriate forms in the same construction. Even after this, one could not always be sure whether the meaning was grasped or whether he was simply generating sentences mechanically and for the pleasure of it. Whatever the case, however, these constructions were rarely grammatically incorrect,

[8] Velten, p. 290.

and sooner or later it would become clear that he was using them under semantically appropriate conditions. Considering the progressive acquisition of new constructions brought the same clarification to his grammatical development as considering the successive acquisition of phonemes brought to the analysis of the phonetic record.

As soon as Stephen began to form constructions of his own, he incorporated English morphemes and words into Garo sentences. He appended Garo suffixes to English words without the least hesitation, but there was never any doubt that his early sentences were Garo, since the morphology and syntax were Garo even if the lexical items came from English. I once heard the sentence *mami laitko tunonaha* 'mommy turned on the light' where the roots of every word were English, but the suffixes (*-ko* 'direct object marker' and *-aha* 'past tense'), word order, and phonology were Garo and there was no doubt that the sentence should be considered a Garo one. When he finally began to form English sentences, he just as readily adopted Garo words into English. This mutual borrowing was made easy partly because his models did the same. All the English speaking adults around him constantly used Garo words in their speech, and Garos borrow readily from English. However many morphemes might be borrowed, there was seldom any question as to which language he was using since affix morphology and syntax were either all Garo or all English. After his vowel systems became differentiated at 2:8, the phonology was also appropriate to the choice of grammatical system. It was only shortly before the systematic separation of the vowels that he used many English sentences. Before that, he spoke with a single linguistic instrument, forged largely from Garo, but with the addition of English vocabulary and a few extra English phones. Later, when he did have two linguistic systems, the two never appeared to interfere with each other. He spoke one language or the other, never a mixture of the two.

One final comparison with Leopold's daughter is worth making. Leopold insists that a striking effect of bilingualism was the looseness of the link between the phonetic word and its meaning.[9] His daughter never insisted on stereotyped wording of stories. Leopold believed that this was due to the fact that she heard the same thing constantly designated by two different phonetic forms, so that form and meaning were not rigidly identified with each other. There can never have been a child more obsessional in this respect than Stephen. When we read to him, he instantly protested the slightest alteration in any familiar text. This was true even though we read to him in his second language, English, where one would suppose the form and meaning to be least rigidly identified. It must have

[9] Leopold, *Language Learning* V, p. 13.

been an idiosyncratic trait rather than bilingualism that freed Leopold's daughter from insistence upon stereotyped wording.

In the fall of 1958, at the age of five and a half, Stephen is attending kindergarten in the United States. He speaks English perhaps a bit more fluently and certainly more continuously than most of his contemporaries. The only Garo words he now uses are the few that have become family property, but I hope that some day it will be possible to take him back to the Garo Hills and to discover whether hidden deep in his unconscious he may not still retain a remnant of his former fluency in Garo that might be re-awakened if he again came in contact with the language.

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The Australian English Vowels as a Diasystem

This paper attempts (1) to argue that certain procedures in what has been called "structural dialectology" have a strong claim to form an essential part of descriptive linguistics, (2) to suggest some procedures in structural dialectology at the phonemic level and (3) to apply the suggested procedures to the description of vowel phonemes in Australian English.

1. Structure and Dialectology

Uriel Weinreich, in attempting to bridge a gap between the methods of descriptive linguistics and those of linguistic geography, obviously conceived his proposals as chiefly benefiting the latter.[1] It is here argued that they can also benefit the former, in that dialectological concepts can deal with certain difficulties inherent in current structural theory and make possible juster methods in description.

1.1. These difficulties arise from the fact that the concept of the unique homogeneous structure has not been reconciled with the actual nature of

[1] Uriel Weinreich, "Is a Structural Dialectology Possible?" *Word* X (1952), 388–400. Other recent studies pertinent to the subject are Edward Stankiewicz, "On Discreteness and Continuity in Structural Dialectology," *Word* XIII (1957), 44–59; Witold Doroszewski, "Le Structuralisme linguistique et les études de géographie dialectale," *Proceedings of the 8th International Congress of Linguists*, 1957, 540–564; Jean Fourquet, "Linguistique structurale et dialectologie," *Fragen und Forschungen im Bereich und Umkreis der germanischen Philologie* (Festgabe Frings), Berlin, 1956, 190–203; J. C. Catford, "Vowel-Systems of Scots Dialects," *Transactions of the Philological Society*, 1957, 107–117; Edward Stankiewicz, "The Phonemic Patterns of the Polish Dialects; a Study in Structural Dialectology," *For Roman Jakobson*, The Hague, 1956, 518–530, and *idem*, "Towards a Phonemic Typology of the Slavic Languages," *American Contributions to the Fourth International Congress of Slavicists, Moscow, September 1958*, The Hague, 1958, 301–319.

speech communities, which are in many, if not all, cases, structurally diversified.

The potential existence of structural diversity in any speech community, however small, is attested by the fact that languages change. Structural diversity is also inevitably present in any community in which there has been, more or less recently, massive dialect mixture. Such mixture may be the result of the displacement of populations or of the mingling of social strata once sharply differentiated. Dialect mixture of both these kinds must have formed an important part of the recent history of modern urban or urbanized speech communities and must be largely responsible for that linguistic diversity so characteristic of these communities yet so little catered for by modern linguistics.[2]

1.2. It is argued by Weinreich,[3] and his arguments are here accepted without further discussion, that partially similar structures can be embraced within a "dialectological" analysis, or 'diasystem'. Does it not follow that the proper description of a structurally diversified speech community—and this means, potentially, any speech community—*demands* such a method, whether the community be divisible into distinct dialect regions or whether the variant structures be freely distributed throughout an area?

Such a method will present the language, not as a unique structure, but as co-existent or 'bracketed' structures. This will encourage the analyst, not to seek out one of the structures as 'the language', but to list variant structures dispassionately. (A similar aim in dialect geography was the object of a method suggested by C. F. Voegelin.[4]) The resultant diasystem can be regarded, not as a mere "scientist's construction,"[5] but as a symbol of what happens when members of this community talk to each other.

At best, the lack of a bracketing method leaves us without a statement of interrelationships between variant structures; at worst it leads to distortions of method and errors of observation. A need to provide an embracing analysis of English undoubtedly lay behind the celebrated proposals of G. L. Trager and H. L. Smith, Jr.[6] But these proposals

[2] For discussion of the neglect of this question, see Glenna Ruth Pickford's comments on the Linguistic Atlas of America in her paper, "American Linguistic Geography: A Sociological Appraisal," *Word* XII (1954), 211–233.

[3] Weinreich, *op. cit.* Also A. Martinet's preface to U. Weinreich, *Languages in Contact*, New York, 1953, xvii.

[4] Cf. Voegelin, "Phonemicizing for Dialect-Study," *Language*, XXXII (1956), 116–135.

[5] Weinreich, *op. cit.* p. 390.

[6] G. L. Trager and H. L. Smith, Jr., *Outline of English Structure*, Norman, Oklahoma, 1951.

attempted to apply to variant structures a method applicable only to unique structures, one at a time. The resultant analysis is unsatisfactory in all sorts of ways (see criticisms by U. Weinreich [*op. cit.*, p. 395] and H. Kurath[7]), and its application to many varieties of English is attended by an abandonment of the basic principles of phonemic analysis. Thus, using Trager and Smith's—albeit dubious—binary analysis of diphthongs and long vowels, Australian English cannot be shown to have more than seven vowels, or Southern British English as described by Daniel Jones more than eight. What has then become of the nine-vowel structure? One cannot seriously consider the view, even though it has been seriously put forward,[8] that some varieties of English "do not use" some of the phonemes which they are assumed to possess.

1.3. In conclusion, it is important to insist upon the distinction made by Weinreich (*op. cit.*, p. 395) when he proposes that the term 'variety' be used for a linguistic structure defined and described as such without reference to non-linguistic criteria such as region or class, leaving languages or dialects as sociolinguistic entities capable of embracing more than one variety.

This term 'variety' is in this paper understood, not as the structure of an idiolect, but as a group structure, i.e., in phonemics, the clustering of idiolectal variants round a set of norms. This implies that if, within a group of persons employing parallel sets of phonemes, the distribution of the variants does not correspond to such a clustering, but reveals for some or all of the phonemes more than one area of concentrated distribution, then there is more than one set of norms and more than one variety of the language. This concept may appear somewhat chimerical, since linguists are not normally in a position to carry out extensive statistical investigations of phoneme variants. It is however necessary to the theory as providing the necessary link in the chain: idiolect–variety–language. In practice there will be a reasonable working assumption that if certain phonetic characteristics of a phoneme set have social meaning in a community, then they constitute evidence for the existence of a separate language variety as here defined.

2. Phonemic Diasystems

Weinreich proposed a method for comparing partially similar phonemic systems (*op. cit.*, pp. 393–395). It is possible to elaborate this method at

[7] Hans Kurath, "The Binary Interpretation of English Vowels: a Critique," *Language* XXXIII (1957), 111–122.

[8] H. A. Gleason, Jr., *Introduction to Descriptive Linguistics*, New York, 1955, p. 32.

sub-phonemic, phonemic and distributional levels of analysis, and to use the results of these procedures in the assessment of the lexical correspondences existing between the varieties in question.

2.1. DISTINGUISHING SUB-PHONEMIC AND PHONEMIC COMPARISON. Weinreich proposes (p. 394) that, in the setting up of a diasystem, if there is a physical difference affecting a phoneme in sets of phonemes which otherwise correspond, then this difference may conveniently be incorporated into the statement of the diasystem. He suggests as an example:

$$_{1,2}//\text{i}\approx\frac{_1\text{e}}{_2\varepsilon}\approx\text{a}\approx\text{o}\approx\text{u}//.$$

If we ask what degree of difference will necessitate this procedure the answer may be difficult to arrive at. It is in fact useful here to distinguish two levels of analysis: one which establishes whether two varieties have parallel sets of phonemes or not, another which examines the physical realizations of the phonemes of such a diasystem. These correspond to two levels of the analysis of a single system: the statement of a phoneme inventory and the description of the allophones. We may therefore distinguish between comparison at the phonemic and sub-phonemic levels, and call the former DIAPHONEMIC, the latter DIAPHONIC comparison. The differentiation of $_1$/e/ and $_2$/ɛ/ in the above example will then be seen as a diaphonic observation, and while it may be useful to incorporate gross diaphonic differences into the formulaic display of diaphonemic comparison, there is also theoretical justification for eliminating such information at this stage. This means that matching phonemes (see 2.2 below) will be symbolized by the same character, regardless of diaphonic differences.

Diaphonemic comparison will establish the equivalence or otherwise, in number and arrangement, of two sets of phonemes; diaphonic comparison, like phonetic description, may be carried to any desirable or possible degree of minuteness.[9]

2.2. DIAPHONEMIC COMPARISON. Weinreich has made it clear, by implication at least, that in comparing different phoneme inventories, it is necessary to establish, around any point where a difference occurs, an area

[9] The term 'diaphonic' appears to be the invention of Daniel Jones, who expounds a theory of diaphones in his *Outline of English Phonetics* (Cambridge, 1956, 53ff.) and gives a fuller account in *The Phoneme* (Cambridge, 1956, 195ff.). His exposition lacks clarity, however, and does not distinguish levels of analysis which in this article are severally labeled diaphonic comparison, diaphonemic comparison, and the establishing of lexical equivalences.

which is to be regarded as affected by, or relevant to, that difference. He does not, however, suggest any principles according to which such delimitations are to be made, and the examples which he offers seem to reveal some inconsistency. Thus his diasystem for Yiddish dialects links a pair of front vowels in two dialects with a single vowel in a third, whereas in a hypothetical diasystem he brackets together a set of four front vowels in one dialect with a set of three in the other, offering no suggestion as to what considerations might determine the choice of one or other of these procedures (*op. cit.*, p. 394). It seems desirable to narrow down to a minimum the scope of such bracketings—if possible to the point where one phoneme in one variety is set against two in the other. Diaphonemic comparison would then involve these steps:

(a) Phonemes will be matched one to one from the two varieties into DIAPHONEMIC PAIRS.

(b) At points of structural difference this one-to-one matching will be impossible. Here DIAPHONEMIC BRACKETS will be established if possible so that one phoneme in one variety will bracket two in the other in a SIMPLE (one–many) BRACKET; if not, so that two or more from one will bracket three or more from the other in a COMPLEX (many–many) BRACKET. A count of the proportions of pairs, simple brackets, and complex brackets will then reveal a certain type of information about the diasystem.

The following suggestions will provide a method of establishing diaphonemic brackets:

(a) A phoneme in one variety has allophones spread over a range which in the other variety is occupied by two phonemes. This will yield a simple bracket. For example, no variety of Australian English distinguishes more than two short central vowels, while many varieties of American English have three. Some way of bracketing $_{Au}$/ə ~ a/ and $_{Am}$/i ~ ə ~ a/ will be sought. $_{Au}$/ə/ ranges from [ɨ] to [ɜ], covering the scope of $_{Am}$/i, ə/, while both Australian and American /a/ lie outside this field. This enables us to put

$$_{Au,\ Am}/\!/\ \frac{_{Au}/ə/}{_{Am}/i \sim ə/} \approx a\ /\!/ .$$

(b) A phoneme in the one variety falls physically between two phonemes in the other. This will also yield a simple bracket. An example of this emerges from a comparison of short vowels of Australian English with those of a variety of Yorkshire English.[10] Each variety has three short

[10] These remarks are based, not on direct study, but on the passage in phonetic notation given in Ida C. Ward, *The Phonetics of English* (Cambridge, 1944), 237–240. The findings are not regarded as having any validity beyond the restricted corpus upon which they are based.

front vowels, while *Au.* has two, *Y.* only one short central vowel phoneme. The phonemes are $_{Au}$/i∼e∼æ∼a∼ə/, $_{Y}$/i∼e∼a∼ə/. Now $_{Au}$/æ/ is [æ], $_{Au}$/a/ is [ʒ]; $_{Au}$/ə/ has been described above. $_{Y}$/a/ is [a], $_{Y}$/ə/ not below lower mid. Thus $_{Y}$/a/ falls between $_{Au}$/æ/ and $_{Au}$/a/ and may be bracketed with them:

$$_{Au,\ Y}/\!/\ \text{i} \approx \text{e} \approx \frac{_{Y}/\text{a}/}{_{Au}/\text{æ} \sim \text{a}/} \approx \text{ə}\ /\!/ .$$

(c) If a series of phonemes in one variety matches physically a parallel series in the other, but one of these varieties has an extra phoneme corresponding to a 'hole' in the other, then this phoneme will be joined with its nearest neighbor and that neighbor's counterpart in the other variety into a simple bracket. If no other single phoneme can be regarded as its nearest neighbor, then no simple bracket can be set up, and a complex bracket must be used. For example, if we find two varieties of French differing in their inventories of nasalized vowels so that *F*1 has /ɛ̃∼ã∼ɔ̃/ while *F*2 has /ɛ̃∼œ̃∼ã∼ɔ̃/ and $_{F2}$/œ̃/ resembles $_{F2}$/ɛ̃/ more closely than it does $_{F2}$/ã/ or $_{F2}$/ɔ̃/, then we can state:

$$_{F1,\ F2}/\!/\ \frac{_{F2}/\tilde{\varepsilon} \sim \tilde{\text{œ}}/}{_{F1}/\tilde{\varepsilon}/} \approx \tilde{\text{a}} \approx \tilde{\text{ɔ}}\ /\!/ .$$

If we then find a further variety of French (*F*3) which has /œ̃/ equidistant between the other three, we must use a complex bracket:

$$_{F1,\ F3}/\!/\ \frac{_{F1}/\tilde{\varepsilon} \sim \tilde{\text{a}} \sim \tilde{\text{ɔ}}/}{_{F2}/\varepsilon \sim \tilde{\text{œ}} \sim \tilde{\text{a}} \sim \tilde{\text{ɔ}}/}\ /\!/ .$$

A diasystem embracing three such systems could evidently not be readily compressed into the scope of a simple formulaic display as has been done till now.

Complex situations may arise if the diasystem involves differences within a phoneme set which can be separated into various subsets, such as short vowel, long vowel, nasalized vowel sets, and it needs to be established whether such subsets should be regarded as independent or not, for the purpose in hand.

Thus in a hypothetical pair of varieties there will be two ways of analyzing a difference within a nasal set of vowels:

(a) $$_{1,2}/\!/\ \text{i} \approx \text{e} \approx \text{u} \approx \text{o} \approx \frac{_{1}/\tilde{\text{i}} \sim \tilde{\text{e}}/}{_{2}/\tilde{\text{e}}/} \approx \tilde{\text{u}} \approx \tilde{\text{o}}\ /\!/$$

(b) $$_{1,2}/\!/\ \frac{_{1}/\tilde{\text{i}} \sim \tilde{\text{ɪ}}/}{_{2}/\tilde{\text{i}}/} \approx \text{e} \approx \tilde{\text{e}} \approx \text{u} \approx \tilde{\text{u}} \approx \text{o} \approx \tilde{\text{o}}\ /\!/ .$$

Solution (a) treats the nasalized set as having some structural independence while solution (b) does not, and it may be that both solutions are equally valid. If this is felt they can be amalgated by complex bracketing:

$$/\!/ \frac{{}_1/\text{i} \sim \text{ĭ} \sim \tilde{\text{e}}/}{{}_2/\text{i} \sim \tilde{\text{e}}/} \approx \text{e} \ldots /\!/ .$$

It may, on the other hand, be possible to establish, not an absolute rightness of one such solution, but a priority, if there are factors present which speak for a certain structural independence of the subsets. This will occur if the members of the subsets have more than a single point of phonetic difference. This will be so in French, for instance, where the nasalized vowels are different in oral formation as well as in nasality from their oral counterparts, both /ẽ/ and /õ/ for instance falling about midway in aperture between two of the oral vowels. This can be regarded as setting the nasalized vowels apart as a structurally independent subset and giving priority in diaphonemic description to the type of treatment accorded them above.

Similarly below (in 3.2) diaphonemic description has given priority to the description of differences within the subsets of vowels, since the opposition of short and long vowels is predominantly not merely short–long, but also monophthong–diphthong. Such priority, however, is a priority only, and the description of lower priority will also be relevant.

2.3. COMPARING PHONEME DISTRIBUTION. Beyond the level of comparing phoneme inventories, structural significance will attach to a comparative listing of permitted phoneme sequences, from which statements as to equivalence or otherwise will arise.

Various possibilities suggest themselves:

(a) The distribution of a phoneme in one variety corresponds exactly with that of its fellow in a diaphonemic pair. This constitutes full distributional equivalence.

(b) Where a phoneme is a member, not of a pair, but of a bracket, there will evidently be non-equivalence. There may, however, be complete parallelism. Suppose, for instance, the bracket:

$${}_{1,2}/\!/ \frac{{}_1/\text{P} \sim \text{P}'/}{{}_2/\text{P}/} /\!/ .$$

Now if ${}_2$/P/ corresponds exactly in distribution with both ${}_1$/P/ and ${}_1$/P′/, there may be said to be distributional parallelism within this bracket, but there will be, for every context in which ${}_2$/P/ occurs, two parallel contexts in Variety 1.

(c) Whenever such equivalence or parallelism is not maintained, statements of difference can be made in the form of differential sequence pairs or of differential sequence brackets. Pairs will arise when there are, in the varieties in question, complementary restrictions on the distribution of two phonemes. Thus if one variety of German has the sequence /#ʃt/ but not the sequence /#st/, while another permits /#st/ but not /#ʃt/, and the two correspond exactly as to what other phonemes can appear in the frame /#–t/, then we can state that $_{G1}$/#ʃt/ and $_{G2}$/#st/ form a differential matching pair. This could be symbolized:

$$_{G1,G2}/\!/\,\frac{_{G1}/\#ʃt/}{_{G2}/\#st/}\,/\!/\,.$$

If it is also found that the phoneme /p/ is similarly affected in both varieties but that the dialects agree in permitting only /s/ before /k/, only /ʃ/ before /v/, this can be symbolized:

$$_{G1,G2}/\!/\,\frac{_{G1}/\#ʃt/}{_{G2}/\#st/}\approx\frac{_{G1}/\#ʃp/}{_{G2}/\#sp/}\approx/\#sk/\approx/\#ʃv/\approx\ldots\,/\!/\,.$$

Sequence brackets will be established where it is found that one-to-one matching of permitted sequences is not possible. Thus Southern British English, as described by Daniel Jones,[11] permits, in morpheme-final position, /i/, /i:/ and /ə/ where Australian English permits only /ii/ and /ə/. Since we can establish a diasystem showing $_{SB,A}$/i:≈i≈ə/, in which the symbol /i:/ equates Jones's /i:/ with the /ii/ described below (3.1), we can then set up a sequence-bracketing formula:

$$_{SB,A}/\!/\,\frac{_{SB}/i\#/\sim/i:\#/}{_{A}/i:\#/}\approx ə\#\,/\!/\,.$$

A recent article by Harary and Paper has suggested that differences in phoneme distribution as between two dialects are susceptible of mathematical evaluation.[12] They base their work on a study of Japanese dialects by B. Bloch who gives in tables a comparison of phoneme distribution in two dialects. Sequence equivalences or brackets as described here could readily be based on these tables. But if, as Harary and Paper themselves observed (*op. cit.*, p. 155), their mathematical procedures are to be complete, they require elaboration which will make them capable of handling sequences of three or more phonemes, since limitations on phoneme distribution often need to be stated in terms of such sequences (cf. the example drawn from German above).

[11] Jones, *Outline*.

[12] Frank Harary and Herbert H. Paper, "Toward a General Calculus of Phonemic Distribution," *Language* XXXIII (1957), 143–169.

2.4. LEXICAL CORRESPONDENCES (at the diaphonemic level). The listing of points of structural difference is completed with the study of phoneme distribution. This will not however yield any lexical CONVERSION FORMULAE[13] or statements of relationships between cognates in the varieties of the diasystem. Such correspondences are important in evaluating the "sameness" or "difference" of two varieties, their degree of reliability may be mathematically computed for a given corpus, and they can be given a structural ranking in terms of the phonemic diasystem within which they operate.

A given lexical correspondence formula is obviously essentially different from a structural match. A structural equivalence such as $_{1,2}//P \approx P' \approx P'' \approx \ldots //$ declares relationships between phoneme inventories, and supposes that future extensions of the corpus are most unlikely to force any revision of the analysis. Such predictability cannot attach to lexical correspondences, and their reliability must be stated as relevant to a given restricted corpus only.

The following procedures are advanced for establishing and assessing statements of lexical correspondence:

(a) RELIABILITY AND RECIPROCITY OF CORRESPONDENCES. If every instance of a phoneme $_A/P/$ in variety A corresponds, in the cognate morpheme in variety B, to an instance of a certain phoneme $_B/P/$, we have a fully RELIABLE SIMPLE correspondence.

$$_A/P/ = _B/P/.$$

If every instance of this same phoneme $_B/P/$ also corresponds to an instance of $_A/P/$ the correspondence is in addition fully RECIPROCAL.

If instances of a given phoneme $_A/P/$ correspond to instances of a series of phonemes $_B/P, P', P'', \ldots/$ in such a way that the context enables us to predict which will be encountered, we have a fully reliable COMPLEX correspondence, which can be expressed as a set of corresponding sequence formulae:

$$_A/XP/ = _B/XP/$$
$$_A/YP/ = _B/YP'/ \text{ etc.}$$

Such correspondences are not reciprocal.

We are left with non-reliable equivalences, when $_A/P/$ will correspond to $_B/P/$, $_B/P'/$, etc., in ways not predictable from the context. A simple count will provide an assessment of the degree of reliability of such correspondences. Thus if $_A/P/$ corresponds to $_B/P/$ and $_B/P'/$ in the proportions

[13] Uriel Weinreich, *Languages in Contact*, New York, 1953, p. 2.

of four to one, we can say that the formula ${}_{A}/P/ = {}_{B}/P/$ is 80% reliable, or, more concisely,

$$ {}_{A}/P/ = \frac{4_{B}/P/}{1_{B}/P'/}. $$

(b) STRUCTURAL STRENGTH OF CORRESPONDENCES. A given lexical phoneme correspondence can be regarded as possessing various degrees of structural strength according to the following method of assessment:

First-grade strength if the corresponding phonemes are members of a diaphonemic pair (i.e. if in ${}_{A}/P/ = {}_{B}/P/$, ${}_{A,B}//P//$).

Second-grade strength if the corresponding phonemes are members of a simple bracket, i.e. if in ${}_{A}/P/ = {}_{B}/P/$ we have

$$ {}_{A,B}\!\!\left/\!\!\left/ \frac{{}_{A}/P/}{{}_{B}/P \sim P'/} \right/\!\!\right/. $$

Third-grade strength if the phonemes are members of a complex bracket.

Low-grade strength will be attributed to correspondences between non-matching phonemes, and such lower strengths can themselves be graded according to their proximity to each other, within the diasystem, of the phonemes involved. Thus if we have a diasystem ${}_{A,B}//i \approx e \approx \varepsilon \approx a//$, then a correspondence ${}_{A}/i/ = {}_{B}/e/$ will be structurally stronger by two stages than ${}_{A}/i/ = {}_{B}/a/$.

These procedures seem to provide a basis for a systematic lexical comparison of dialects. The data they provide can be presented in graphs or tables. Whether these can be usefully submitted to more sophisticated mathematical treatment, the present writer cannot surmise.

There follow in illustration some examples of lexical correspondences involving the short vowels of Yorkshire (as defined above, 2.2 (b)) and Australian English.

Having set up the partial diasystem

$$ {}_{Y,A}\!\!\left/\!\!\left/ i \approx e \approx \frac{{}_{Y}/a/}{{}_{A}/æ \sim a/} \approx o \approx u \right/\!\!\right/ $$

we can find in the Yorkshire text 21 examples of /a/, of which 17 correspond to ${}_{A}/æ/$, two to ${}_{A}/a:/$, one to ${}_{A}/ai/$, one to ${}_{A}/ei/$. If on the other hand we examine the occurrence of ${}_{A}/æ/$ in an Australian version of this text, we find only the 17 examples involved in the above count. Thus

$$ {}_{Y}/a/ = {}_{A}\begin{cases} 17/æ/ \\ 2/a:/ \\ 1/ai/ \\ 1/ei/ \end{cases} \quad \text{but } {}_{A}/æ/ = {}_{Y}/a/. $$

If we now consider instances of $_{Y}$/u/, we find

$$_{Y}/u/ = {}_{A}\begin{cases} 14/a/ \\ 4/u/ \\ 2/o/ \end{cases}$$

Grading the structural strength of these correspondences, we shall state that $_{Y}$/a/ = $_{A}$/æ/ has second-grade strength while the other three correspondences with $_{Y}$/a/ are of low grade. $_{Y}$/u/ = $_{A}$/u/, on the other hand, is of first-grade strength, while $_{Y}$/u/ = $_{A}$/o/ is of low grade, though one stage stronger than $_{Y}$/u/ = $_{A}$/a/.

3. The Syllabic Phonemes of Australian English

A phonemic analysis of the syllabic phonemes in varieties or presumptive varieties of Australian English is followed by a diasystem description incorporating these varieties.

3.01. It is important that before dialectological procedures of the type described above are undertaken, phoneme inventories be satisfactorily established. In cases of dubious phonemicization the choice of one or other of the available analyses can have a considerable effect on the eventual diasystem. This is the case with Australian English, and it is proposed to discuss the phonemic analysis of three varieties of Australian English before discussing their incorporation into a diasystem.

It is emphasized that these three varieties are not by any means the only ones that can be observed. They have all been observed in frequent occurrence by the writer. They will be symbolized as *A*1 (corresponding approximately to A. G. Mitchell's "Educated Australian"),[14] *A*2 and *A*3 (corresponding to two varieties of Mitchell's "Broad Australian"). It is assumed for purposes of discussion that they are separate varieties, but they may not in fact be so. (See 3.4.)

3.02. The phonetic notation used is that of the I.P.A. modified by the inclusion of cardinal symbols for central colors as follows:

	Spread:	Rounded:
High	ɨ	ʉ
Higher mid	ɘ	ɵ
Lower mid	ɜ	
Low	ɐ	

[14] A. G. Mitchell, *The Pronunciation of English in Australia*, Sydney, 1946; *Spoken English*, London, 1957.

Further,

[˛] indicates a color half-way to that of the next-lower vowel, but [æ] is used for the color between [ɛ] and [a].

[˃] or [˂] indicate a half-centered color.

[ᵘ] indicates non-syllabicity.

The values of this system are based on those exemplified by a recording made by Daniel Jones.[15]

3.10. The non-syllabic phonemes of Australian English form a series exactly parallel with those of Southern English as described by Daniel Jones.[16] This statement holds for all the contoids and for /h/, /j/ and /w/ in syllable-initial position. The following discussion will handle all the syllabics as they occur with or without a following non-syllabic vocoid extension, be this length-mora or glide.

3.11. INVENTORY OF VOWELS. All the phonemically distinct segments of this kind are shown, for *A*1 and *A*2, in the following word-list in phonetic representation. (The words are represented as pronounced alone, i.e. with phrase-final stress.)

Word	(1) *bid*	(2) *bead*	(3) *beard*	(4) *bed*	(5) *bared*
*A*1:	bi˃d	bi˃ĭd	bi˃ə̆d	bɐ̨d	bɐ̨ə̆d
*A*2:	bi˃d	bi̯ĭd	bi˃ə̆d *or* bi˃:əd	bed	beə̆d

Word	(6) *bayed*	(7) *bad*	(8) *(cup)board*	(9) *bird*	(10) *bud*
*A*1:	bɛĭd	bæ:d	kɤ̨bəd	bɐ̨:d	bɐ̨d
*A*2:	bɐ̨id	bɛ:d	kɤ̨˂bəd	bə:d	bɐ̨˂d

Word	(11) *bard*	(12) *bide*	(13) *bowed*	(14) *bod* ('person')
*A*1:	ba:d	ba˂ɛ̆˃d	ba˃ŭ˃d	bɒd
*A*2:	ba˃:d	bɒɛ̆˃d	bæə̆d	,,

Word	(15) *buoyed*	(16) *board*	(17) *bode*	(18) *lured*
*A*1:	bɒi̯˃d	bɒə̆d *or* bɒ:d	bɒ˂ŭ˂d	lɥəd
*A*2:	boi̯˃d	bo:d	bɐ̨ŭd	

[15] Daniel Jones, *Cardinal Vowels* (Linguaphone DAJO.1D,2G—undated).

[16] Jones, *Outline*.

Word	(19) *pud* ('Fatty')	(25) *booed*
*A*1:	pu̯ᵊd	buᵊŭd
*A*2:	,,	biŭd
		or buŭd

Variety *A*2 has, for no. 18, the same vocoid segment as for no. 16.

3.12. IDENTIFYING SHORT-VOWEL PHONEMES. These segments are subject to the following positional variations:

(1) *Retractions.* There is slight retraction before velar contoids and substantial retraction before final or prefinal /l/, which is strongly velarized.

(2) *Length Variations.*

(a) The short syllabics of *bid* (no. 1), *bed* (no. 4), *bud* (no. 10), *bod* (no. 14), *pud* (no. 19) are slightly shortened before an unvoiced contoid.

(b) The long syllabic of *bad* (no. 7) is greatly shortened before unvoiced contoid, being in this position comparable in length with those of the group (a).

(c) The other segments except that of the second syllable of *cupboard* (no. 8), i.e. all the remaining long sequences, are substantially reduced in length before an unvoiced contoid, but maintain in this position a markedly longer character than those grouped under (a) and (b) above. In the open phrase-final syllable these are all lengthened. They also show length variation according to stress, the most strongly stressed positions producing greatest length.

The segments of no. 2, *bead* and no. 20, *booed*, are, in *A*1 only, in positional variation with a short [i] and [uᵊ] respectively occurring in phrase-internal position before an unvoiced contoid (as in *seating-accommodation, suit and hat*). These short vocoids are then distinguished chiefly by color from the more open, but only minutely shorter, [i˃] and [u̯ᵊ] which represent nos. 1 and 19 in like context: i.e. as in *sitting-accommodation* and *soot and ashes.*

The syllabics listed under (a) and (b) are restricted to occurrence in a syllable closed by a contoid, and readily yield an unequivocal set of six short syllabic phonemes for each variety, namely: *bid* /i/; *bed* /e/; *bad* /æ/; *bud* /a/; *bod* /o/; *pudding* /u/.

Special consideration must be given to no. 8, the unstressed vocoid of the second syllable of *cupboard.* This is in positional variation in unstressed syllables with a number of central colors ranging from [ɜ] to [i̞] according to the context, so that the highest versions occur between lingual contoids (as in *detect, chases*), the lowest in initial or final position (as in *appear,*

chaser). It is not possible to find phonemic opposition between any of these nor is it possible to regard them as unstressed allophones of any of the other syllabics. The only reasonable possibilities for this sort of compression are presented by the vocoids of *bud*, *bid* and *bird*, and phonemic opposition with these in unstressed position is proved by the following pairs:

pickaback	[pi̯ˀkɨ̯bak]	*hiccuping*	[hi̯ˀkəpiŋ]
endure	[əndjú̯ə]	*undo*	[ʒndú˂ŭ]
bannock	(bǽnɨ̯k]	*panic*	[pǽnik]
catarrh	[kɨ̯tá:]	*curtail*	[kətéĭl]

(Notations represent *A*1, but parallel differences occur in *A*2 and *A*3.)

It is thus necessary to recognize a seventh short syllabic phoneme which is limited to occurrence in unstressed syllables. This is labeled /ə/.

3.13. ANALYZING THE LONG SEGMENTS. This leaves for consideration the long and gliding syllabics. In the light of current controversies the phonemic analysis of these long sequences must be carefully considered. They may be treated as unit phonemes or as sequences of two phonemes, the first of which is in each case an allophone of one of the short vowels already identified. Before considering this question, however, a certain internal organization of these sequences must be observed. They fall phonemically into two main groups:

(a) long monophthongs and ingliding diphthongs;
(b) out-gliding diphthongs.

The first of these two groups must be regarded as phonemically unified, since length-mora is nowhere opposed to in-glide, and is at times in positional or free variation with it. The internal patterning of this subset is however not symmetrical in any of the three varieties. Thus in *A*1, *beard* represents a class of morphemes with ingliding diphthong only, while the diphthong of *bared* is in positional variation with an [ę:] which occurs before /r/, as in *fairy* [fę̆:ri˃ĭ]. Long monophthongs only represent the central colors as in *bird* and *bard*, while in words like *board* there is, as indicated, free variation between monophthong and diphthong. This free variation occurs only in phrase-final stressed position of maximum length, the monophthong only occurring in positions of lesser stress and/or diminished length (as in *Board of Works*, *bought*). *Lured* represents a class of morphemes whose diphthong is in free variation with the monophthong [ų:] before /r/, as in *boorish*. In *A*2 a similarly asymmetric distinction occurs, though here the monophthong is more frequent than the diphthong.

Beard has a diphthong, but in free variation with a monophthong followed by syllabic [ə], this latter sequence being limited to phrase-final position and more frequent in the open syllable (e.g. *beer*). On the other hand [i̯ɔ:] is normal before /r/. The diphthong of *bared* appears as a long monophthong under lesser degrees of stress, while the monophthong of *board* is unchanged in other contexts.

Mention was made above of a third variety, *A*3. It differs from *A*2 only in this part of the system, presenting no ingliding diphthongs at all, but only long monophthongs. Phrase-final stress is accompanied in *A*3 by the addition to this monophthong of syllabic [ə].

It should be noted here that *A*2 and *A*3 have no phonemically separate segment corresponding to the [u̯ə̆] of *A*1 (*lured*, no. 18).

It is thus apparent that length-mora and in-glide nowhere constitute a phonemic opposition. It has been noted that in *A*2 and *A*3 there is free or positional variation between [ə̆] and [:]. There is however a limited phonemic opposition between [ə̆] and [ə], which is proved by such a pair as *A*1 *drawer*[1] (in *a chest of drawers*): [drɒ:] or [drɒə̆] or [dró:ə], and *drawer*[2] 'one who draws': [dró:ə] only. The opposition is less convincingly present in *A*2 and *A*3, where *drawer*[1] is [dro:] and *drawer*[2] [dró:ə]. Even here, however, the avoidance of undue discongruence of allophone patterning is held to justify the retention of /ə/ as a syllabic phoneme not to be regarded as an allophone of the length-mora.

Words such as *tower*, *fire*, are dissyllabic.

We have then, if these sequences are to be treated as pairs comprising a vowel and semivowel, three semivowel phonemes to identify; these presumably would be /j/, /w/ and a third, manifesting itself as [:] or as [ə̆] in various contexts.

Can [:] be equated, like /j/ and /w/, with a prevocalic feature with which it is in complementary distribution? There appear to be two prevocalic features—[h] and [r]—with which our [ə̆, :] feature might be bracketed, each having roughly equal claims and disadvantages. The latter are great: each has a strong phonetic feature (voicelessness in one, retroflexion in the other) which is not in any way matched in prevocalic /j/ and /w/. It is also possible to discover rare cases in which both [h] and [r] appear in oppositive distribution with [ə̆, :]. Thus *A*1 *The devil's moral arms* and *The Devil's maw alarms* contain the sequences [mɒ́rəl] and [mɒ:əl] respectively, where [:] and [r] are in distinctive opposition. Similarly *her own* may appear as [hə:ɒ̨˂ŭn], while *her roan* is [hərɒ̨˂ŭn]. These curious marginal cases might be dealt with by the invocation of stylistic or junctural factors if all other indications were positive, but the total picture that emerges is one of multiple discongruities and asymmetries. The very fact that two

possible solutions exist and that both are dubious, encourages us to reject both.

This leaves us with the possibility of regarding the second part of these complex sequences as a phoneme in its own right, manifested as [:] or as [ə̃] according to the context. This of course involves a major discongruity in the distribution of the semivowels, since our new phoneme will appear only post-vocalically. Accepting for the moment this proposal, we can use the symbol /:/ for this phoneme and tentatively propose for *A*1 *beard*= /bi:d/, *bared*=/be:d/, *bird*=/bə:d/, *bard*=/ba:d/, *board*=/bo:d/, *boor*= /bu:/. This will produce a slight discongruity of allophone patterning in the vowels, since /:/ will be found not to affect any of them except /a/ which it will open, and /o/, which it will substantially close. Since in *A*2, *A*3 the [o:] of *board* is much closer to the [ʊ˂] of *put* than it is to the [ɒ] of *bod*, it seems necessary to interpret *A*2, *A*3 *board* as [bu:d]. The only alternative to this is to discover that there are three phonemes, /u/, /o/ and /ə/, /ɔ/ being limited to words like *bod*, while /o/ never occurs alone, but always before one of the semivowels: an unhappy choice, since /ə/ and /o/ will be seen to be in completely complementary distribution, and are syllabics of neighboring color!

If we now examine the implications of a two-phoneme analysis of the out-gliding diphthongs, even stronger grounds for uneasiness appear. *A*1 *bead* will contain /i/+/j/, *bay* /e/+/j/, *buy* /a/+/j/, *boy* /o/+/j/, interpreting the nucleus each time as an allophone of the short-vowel phoneme most resembling it. This entails a slight opening of /e/, retraction and opening of /a/, closing of /o/. *A*2 and *A*3 *bead* will be /ə/+/j/, *bay* /a/+/j/, *buy* /o/+/j/, *boy* /u/+/j/, with only /u/ disturbed, since it is now lowered slightly.

*A*1 *booed* will yield /u/+/w/, *bode* /o/+/w/, *bowed* /a/+/w/. Here we find that /w/ fronts /u/, fronts and closes /o/, by which it is itself fronted, and opens /a/.

*A*2 and *A*3 *booed* yields /ə/+/w/, *bode* /a/+/w/, *bowed* /æ/+/w/. Here /w/ does not affect /a/, but is fronted by it, does not affect /æ/ but is opened by it.

None of these sets yields a congruent patterning of allophones for either *A*1, *A*2 or *A*3.

The binary (two-phoneme) analysis of these sequences then appears possible only if a fair amount of distributional irregularity and a large amount of discongruence of allophone patterning is considered acceptable. This must be a matter of taste, since it cannot be laid down as a principle that only complete congruity of allophone patterning is admissible. (Such a principle would, for instance, force the vowels of *back* [æ] and *bag* [æ:]

into separate phonemes.) The present writer prefers to reject the two-phoneme analysis in favor of the unit-phoneme analysis, sacrificing smallness and symmetry of the phoneme inventory for a high level of pattern congruency and of distributional regularity of the phonemes. He is in no doubt of the wisdom of this choice, but as many linguists today are firmly attached to the two-phoneme analysis for English, it seems advisable to examine both types of analysis in the following paragraphs, which will consider the setting up of a diasystem for these varieties of Australian English.

3.2. An Australian Diasystem.

3.21. The Phonemes. The following phoneme inventories would then be the basis of dialectological comparison:

Unit-Phoneme Analysis.

*A*1 Short: /i∼e∼æ∼a∼ɔ∼o∼u/
Long and/or ingliding: /i:∼e:∼a:∼ɔ:∼o:∼u:/
Outgliding: /ii∼ei∼ai∼oi/∼/uu∼ou∼au/

*A*2 Short: /i∼e∼æ∼a∼ɔ∼o∼u/
Long and/or ingliding: /i:∼e:∼a:∼ɔ:∼o:/
Outgliding: /ɔi∼ai∼ɔi∼oi/∼/ɔu∼au∼æu/

*A*3 *A*3 differs from *A*2 only at the diaphonic level and therefore requires no separate treatment here.

Two-Phoneme Analysis.

*A*1 Vowels: /i∼e∼æ∼a∼ɔ∼o∼u/

*A*2 Semivowels: /j∼w∼:/

Diaphonemic formulae would then be:

Short: $_{A1,A2}//i \approx e \approx æ \approx a \approx ɔ \approx o \approx u//$

This formula will be valid for both the unit-phoneme and the two-phoneme analyses. If the unit-phoneme analysis is adopted the long and complex nuclei will be further handled as follows:

Long and/or ingliding:

$$_{A1,A2}\!/\!/\, i: \approx e: \approx a: \approx ɔ: \approx \frac{_{A1}/o: \sim u:/}{_{A2}/o:/} \,/\!/ .$$

This statement is, for reasons stated above (2.2), given priority over the alternative:

$$_{A1,A2}\!/\!/\, o \approx ou \approx o: \approx \frac{_{1}/u \sim u:/}{_{2}/u/} \approx uu \,/\!/ .$$

Outgliding: $_{A1,A2}$//ii≈ei≈ai≈oi≈uu≈ou≈au//, which is diaphonically:

$$_{A1,A2}\!/\!/\frac{_1\text{ii}}{_2\text{əi}}\approx\frac{_1\text{ei}}{_2\text{ai}}\approx\frac{_1\text{ai}}{_2\text{əi}}\approx\frac{_1\text{oi}}{_2\text{oi}}\approx\frac{_1\text{uu}}{_2\text{əu}}\approx\frac{_1\text{ou}}{_2\text{au}}\approx\frac{_1\text{au}}{_2\text{æo}}\!/\!/.$$

This last set incorporates a substantial degree of diaphonic divergence within each diaphonemic pair. It is a perfectly valid set of diaphonemic pairs, since it brackets two seven-member sets whose internal structures match. Thus $_{A1}$/ii/ differs from $_{A1}$/ei/ in a higher point of origin, and from $_{A1}$/oi/ in a more forward and unrounded point of origin. Exactly the same points of difference account for the opposition between $_{A2}$/əi/, $_{A2}$/ai/ and $_{A2}$/oi/, and the reader can readily satisfy himself that similar observations can be made for all the other members of these sets.

If however we adopt the two-phoneme analysis, we shall not at this stage be concerned with the complex sequences except insofar as they require the listing of semivowels:

$$_{A1,A2}\text{//j}\approx\text{w}\approx\text{:}\text{//}.$$

Thus at the level of the phoneme inventory, the unit-phoneme analysis will yield diaphonemic equivalence except at one point, while the binary analysis will yield complete equivalence.

3.2.2. STRUCTURAL SEQUENCE MATCHING. The unit-phoneme analysis once adopted, there are no differences in permitted phoneme sequences to be observed. If however we use the binary analysis, the necessary observations will be fairly complex.

For the long and/or ingliding sequences we can state fairly simply:

$$_{A1,A2}\!/\!/\text{i:}\approx\text{e:}\approx\text{a:}\approx\text{ə:}\approx\frac{_{A1}\text{/o:}\sim\text{u:/}}{_{A2}\text{/u:/}}\!/\!/.$$

For the outgliding sequences the position will be much less simple. *A*1 permits only /ij/, /ej/, /aj/ and /oj/, *A*2 only /əj/, /aj/, /oj/ and /uj/. Applying the principles outlined above we get:

$$_{A1,A2}\!/\!/\frac{_{A1}\text{/ij}\sim\text{ej/}}{_{A2}\text{/əj/}}\approx\text{aj}\approx\frac{_{A1}\text{/oj/}}{_{A2}\text{/oj}\sim\text{uj/}}\!/\!/.$$

At this stage the pictures given by the two basic analyses develop considerable discrepancies.

3.23. LEXICAL CORRESPONDENCES. The establishing of lexical correspondences produces very different results according as the unit-phoneme or the two-phoneme analysis is adopted. The unit-phoneme analysis yields

a very high proportion of correspondences of maximum structural strength and of high reliability. Thus the word *day*, *A*1/dei/ *A*2/dai/, is found to entail a correspondence within the one diaphonemic pair, and an overwhelming number of words will correspond similarly within this pair. The same is true of any of the other vowels.

If on the other hand the diasystem is based on the binary analysis, an immense number of correspondences involving the diphthongs, like *A*1/biit/ = *A*2/bəit/, will be, though highly reliable and reciprocal, yet of low structural strength. Since, however, there is in this part of the diasystem a set of structural sequence matches, it is possible to make these the basis of a slightly different assessment of structural strength, thus:

*A*1/ij/ = *A*2/əj/ (third-grade)
*A*1/ej/ = *A*2/aj/ (low-grade)
*A*1/aj/ = *A*2/oj/ (low-grade)
*A*1/oj/ = *A*2/uj/ (third-grade).

It thus emerges that the diasystem based on the unit-phoneme analysis presents a simpler basis for comparison of the two varieties of English, concentrating the points of difference at the diaphonic and diaphonemic levels. The binary analysis on the other hand throws the points of difference into the distributional and lexical levels of analysis. The former thus suggests a highly uniform structure with strong diaphonic variation. The latter does not even escape the need to describe diaphonic variation, since the vowel-allophones before semivowels require such treatment.

The results of the dialectological comparison thus seem to exert pressure towards the choice of the unit-phoneme analysis.[17]

3.3. FURTHER VARIETIES. It was stated above that other varieties of Australian English have been observed. These differ only in minor degrees from those already described. The present writer's speech exemplifies a frequent pattern in that his forward-gliding diphthongs are those of *A*1 while his backward-gliding ones are those of *A*2. Another very frequent pattern combines the backward-gliding /əu/ of *A*2 with the /ou/ and /au/ of *A*1. Further variation produces varying patterns within the long monophthong-ingliding set, so that positionally determined distribution of

[17] Concerning the lexical equivalences relevant to the presence or absence of /u:/ in Australian English, we might note the following: *A*1/u:/ generally corresponds to *A*2/o:/ in syllables which can bear stress, except when preceded by /j/, in which case *A*2 has /əuə/. In unstressed syllables, as in *actual* (*A*1/æktju:l), *A*2 in general has /əuə/ or /əwə/, sequences which may be regarded as phonemically distinct.

length-mora and inglide is subject to fairly considerable variation from variety to variety. Provided, however, that the unit-phoneme analysis of complex nuclear sequences be adopted, such variation rests at the diaphonic level.

3.4. CONCLUDING REMARKS. It is evident from the above that the unit-phoneme analysis of the vowels of Australian English reduces almost all variation to the diaphonic level and leaves as the only observable variation in phoneme inventory the presence or absence of the phoneme /u:/. Now it is implicit in the exposition above (Sections 1 and 2) that if two idiolects have matching phoneme inventories, any observable difference between the realizations of these phonemes in the two idiolects cannot, without wider observation of the whole speech community, be characterized as a difference due to differing realizations of the same set of norms or as diaphonic variation due to an approximation of each idiolect to a different set of norms. Whichever solution be correct, the effective situation as between the two speakers will be little affected, and an analysis of the phonemes of the one speaker will be applicable to those of the other. A speech community like the Australian one, in which variation is chiefly or exclusively diaphonic, can usefully be described in terms of a unique structure: practical applications of the description, as in word-lists or in the teaching of the language or of its analysis, can then make use of a phonemic analysis applicable to the whole. *A*1, it seems, is a separate variety: not only is there a difference of phonemic structure, but the diaphonic differences characterizing its diphthongs also have, as A. G. Mitchell states, social meaning in terms of economic or educational background.[18] *A*2 and *A*3 are here treated as separate varieties, but it remains uncertain whether they are separate varieties or variants of the same variety.

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[18] Mitchell, *Pronunciation*, 12–18 et al.

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Predicate Adjective Usage in Standard Russian

Introduction

The aim of this paper is to describe predicate adjective usage in modern Standard Russian. The main corpus consisted of a considerable amount of written Russian as examined in the newspaper *Pravda* and the periodical *Krokodil*. The former uses mostly a neutral literary style; the latter contains much conversational material. Most of the examples given in this paper were drawn from these two sources.[1] To save space, many of the sentences have been shortened. Other examples cited in this study were checked by five highly literate native informants. In addition, these informants were utilized in an attempt to establish the existence of variants and any semantic differentiation between such variants.

Russian predicate adjectives may appear in one of three forms: short, long nominative, and instrumental (designated here as *S*, *L*, and *I*, respectively). In a description of their usage, it is necessary to take into account not only the type of adjective, the copulative verb, and the subject of the sentence in which it appears, but also the effect of certain other syntactical factors, of lexicological-semantic factors, and of style considerations.

Russian predicate adjectives occur in what Bloomfield calls 'equational sentences'.[2] This sentence-type consists essentially of a subject noun or pronoun, copulative verb, and a predicate adjective (or noun or pronoun; predicate nouns and pronouns do not concern us here). All the examples

[1] Previous treatments of this question have been based primarily on examples taken from Russian belles-lettres: J. Fontenoy, "De l'adjectif attribut au nominatif avec la copule *bytj*," *Mélanges pour Paul Boyer* (Paris, 1925), 362–374; A. Mazon, "L'Attribut en russe moderne littéraire," *Zbornik* for A. Belić (Belgrade, 1937), 370–391; V. Vinogradov, *Russkij jazyk* (Moscow–Leningrad, 1947), 262–270 and 402–405; M. Benson, "The Development of Predicate Adjective Usage in Russian Literary Prose from Pushkin On" (Ann Arbor, 1954), Microfilm Publication No. 8535; U.S.S.R. Academy of Sciences, *Grammatika russkogo jazyka*, 2 vols. (Moscow, 1953–1954), II, 445–468.

[2] *Language* (New York, 1933), 172f.

cited below belong to this sentence-type. We shall begin the description of usage with basic sentences, containing the present tense of *byt'* (=zero) 'to be' as copula.[3] In the following sections, other factors will be considered.

1. Basic Sentences With Zero as Copula

1.1. The corpus revealed that many adjectives are either *L* or *S*. These adjectives will be called Class I in this study. Examples: *on slabyj* or *slab* 'he is weak'; *kniga interesnaja* or *interesna* 'the book is interesting'; *pol čistyj* or *čist* 'the floor is clean'; *plat'e krasivoe* or *krasivo* 'the dress is pretty'; *urok očen' trudnyj* or *truden* 'the lesson is very difficult'; *povestka dnja obširnaja* or *obširna* 'the agenda is vast'; *itogi polëta otličnye* or *otlični* 'the results of the flight are excellent'; *ètot priëm novyj* or *nov* 'this method is new'; *dvorec grandioznyj* or *grandiozen* 'the palace is enormous'; *Ivan umnyj* or *uměn* 'Ivan is intelligent'; *takoe povedenie neprilično* or *neprilično* 'such behavior is not decent'; *otvet jasnyj* or *jasen* 'the reply is clear'; *uščerb neznačitel'nyj* or *neznačitelen* 'the damage is slight'. The informants were unable to establish any clear semantic difference between *L* and *S* in such sentences. The forms just cited can be considered synonymous. Thus, the assertion often made in normative works on Russian grammar that *L* denotes a permanent quality and *S* a temporary quality[4] was found often not to be valid. However, in certain instances such a distinction does exist. In addition, *L* forms are sometimes felt to be partly substantivized.[5] Examples: *on bol'noj* 'he is sick' (always) or 'he is a sick man' but *on bolen* 'he is sick' (now); *ona vesëlaja* 'she is a gay person' but *ona vesela* 'she is gay' (now). The time modifier may, of course, be expressed: *segodnja ja svoboden* 'today I am free'; *teper' ix radost' bespredel'na* 'now their joy is supreme'. To be sure, examples of *L* usage when a temporary quality is clearly expressed or understood do occur: *rubaška eščë mokraja* 'the shirt is still wet'; *dela teper' tjažëlye* 'the situation is difficult now'; *pol čistyj* 'the floor is clean' (now). In the proper context, the latter sentence could have the meaning 'the floor is a clean one' (i.e., always clean).

1.1.1. Both the corpus and previous investigators agree that for Class I adjectives, *L* is used more frequently in conversation.[6] Conversely, *S* can

[3] The zero copula may be marked in speech by a non-distinctive pause before *L*. See Vinogradov, 265, and Bloomfield, 185.

[4] For example, A. N. Gvozdev, *Očerki po stilistike russkogo jazyka* (Moscow, 1952), 101.

[5] See Vinogradov, 268.

[6] For example, see Fontenoy, 368–370.

be characteristic of a literary style.[7] In view of the fact that *L* (and *I*) usage has greatly increased in the last hundred years, it is not surprising that *S* can sometimes appear bookish and obsolescent, recalling the syntax of Pushkin-era literature.[8] (See sections 3.5.1.1, 4.4.5, and 4.5.1.1.) Interesting confirmation of the literary character of *S* for Class I has been recently provided by a report of a Soviet school teacher. He points out that young Russian children use *S* forms rarely until they have mastered the literary language in school.[9]

1.1.2. Certain adjectives are always *L* in some semantic situations and *S* in others:[10] *naše delo pravoe* 'our cause is just' (but *on prav* 'he is right'); *ceny tvërdye* 'prices are fixed' (but *on tvërd v matematike* 'he is good in math'); *ètot zvuk gluxoj* 'this sound is unvoiced' (but *on glux k vsem pros'bam* 'he turns a deaf ear to all requests'); *rebënok živoj* 'the child is lively' (but *on živ* 'he is alive'); *korzinka polna* 'the basket is full' (but *ona polnaja* 'she is stout'); *on plox* 'he's in poor health' (but *ètot restoran ploxoj* (or *plox*) 'this restaurant is bad'); *ona xoroša* 'she is pretty' (but *ona xorošaja* 'she is a good person').

On participles, see also section 1.3.1.

1.2. Certain adjectives are always *L*. These will be called Class II. Many have no corresponding *S* at all: *dom bol'šoj* 'the house is large'; *traktory amerikanskie* 'the tractors are American'; *predprijatie gosudarstvennoe* 'the enterprise is state-owned'; *ix texnika peredovaja* 'their technology is advanced'; *štraf grošovyj* 'the fine is trifling'; *pol zemljanoj* 'it's a dirt floor'; *vxod platnyj* 'there is an admission fee'; *oni kak čužie* 'they are like strangers'. Superlative forms in *-šij* are included here: *ta reka dlinnejšaja* 'that river is the longest'.

1.3. Certain adjectives are always *S*. These will be called Class III.[11] Several lack *L* completely: *rad* 'glad', *gorazd* 'capable' and simple

[7] For further discussion see A. M. Peškovskij, *Russkij sintaksis v naučnom osveščenii*, 7th ed. (Moscow, 1956), 225f, and Vinogradov, 264f.

[8] The increase of *L* and *I* usage has been discussed by several investigators. See, for example, Academy, *Grammatika*, II, 445 and A. A. Šaxmatov, *Sintaksis russkogo jazyka*, 2 vols. (Leningrad, 1925), I, 178. Šaxmatov also points here to the disappearance of semantic differentiation between *L* and *S* in popular speech. For the obsolescent character of *S* in certain constructions, see Academy, *Grammatika*, II, 453–455.

[9] G. F. Mjasnikov, "Stilističeskoe ispol'zovanie kratkix i polnyx prilagatel'nyx v sostave skazuemogo," *Russkij jazyk v škole*, 1958, no. 1, 14.

[10] See also N. Švedova, "Polnye i kratkie formy imën prilagatel'nyx v sostave skazuemogo v sovremennom russkom literaturnon jazyke," *Učënye zapiski*, Moscow University, CL (Moscow, 1952), 74ff., and Vinogradov, 262f.

[11] In substandard Russian, Class III may be *L*. See, for example, Academy, *Grammatika*, II, 459.

comparatives such as *novee* 'newer'. Most, however, do possess a corresponding *L*, which is used attributively. Examples of predicate usage: *očerk gotov* 'the outline is ready'; *den'gi ne nužny* 'money is not needed'; *oni dovol'ny* 'they are content'; *Griša ne soglasen* 'Grisha does not agree'; *muzyka slyšna* 'music can be heard'; *gorod viden* 'the city is visible'; *my dolžny* 'we must'; *oni poxoži drug na druga* 'they look alike'; *kto vinoven?* 'who is guilty?'

1.3.1. Past passive participles are normally Class III: *magaziny zakryty* 'the stores are closed'; *zamók isporčen* 'the lock is broken'; *soldat ranen* 'the soldier has been wounded'; *zdanie postroeno* 'the building has been built'. However, a few words of participial origin have become Class I adjectives: *professora rassejannye* (or *rassejanny*) 'the professors are absent-minded' and *vražeskie polki rassejany* 'the enemy regiments have been scattered'; *èti ljudi ograničennye* (or *ograničenny*) 'these people are narrow-minded' and *naši sredstva ograničeny* 'our resources are limited'.[12]

1.3.2. Certain adjectives which are Class III in the predicate function have a different meaning when used attributively: *vidnyj inžener* 'a prominent engineer' (vs. *viden* 'visible'); *dolžnym obrazom* 'in a suitable manner', *dolžnyj vklad* 'a proper contribution' (vs. *dolžen* 'obliged'); *gotovoe plat'e* 'ready-made clothing' (vs. *gotov* 'ready').

1.4. The intensifiers *kakoj* 'what a', 'so', *takoj* 'such a' and *samyj* 'the most' may stand before *L*; *kak* 'how' and *tak* 'so' may precede *S*: *kakoj on vysokij!* 'he is so tall!'; *on takoj lenivyj* 'he is such a lazy one'; *on takoj bol'šoj* 'he is such a big fellow'; *ètot pavil'on samyj interesnyj* 'this pavilion is the most interesting (one)'; *kak ona krasiva!* 'how pretty she is!'; *on ne znal, čto artisty tak populjarny* 'he didn't know that actors were so popular'; *my tak dovol'ny* 'we're so satisfied'.

2. Special Lexicological-Semantic Factors

2.1. The *S* form of adjectives denoting measurements or age can have the additional meaning of 'too':[13] *botinki veliki* 'the shoes are too large'; *brjuki široki* 'the trousers are too wide'; *on ešče molod* 'he is still too young' (cf. *on molodoj* 'he is young'); *ulica uzka* 'the street is too narrow' (cf. *ulica uzkaja* 'the street is narrow'); *rukava korotki* 'the sleeves are too short'; *komnata mala* 'the room is too small'. However, in certain contexts the

[12] See also *ibid.*, I, 305–307, and Vinogradov, 278–284.

[13] When *sliškom* 'too' is expressed, *S* or *L* can follow: *ètot stol sliškom nizok* or *nizkij* 'this table is too low'.

implication of 'too' is absent: *nasledie Čajkovskogo veliko* 'Tchaikovsky's heritage is great'; *ėta skam'ja dostatočno široka* 'this bench is wide enough'; *ix moral'nye kačestva vysoki* 'their moral qualities are high'. Perhaps a larger corpus would make possible a hypothesis as to the kinds of context which imply and do not imply 'too'.

2.2. Certain adjectives which are often used attributively with specific nouns in fixed expressions usually remain *L* when shifted to the predicate function: *položenie beznadëžnoe* 'the situation is hopeless'; *porjadok slov svobodnyj* 'word-order is free'; *vremja trudnoe* 'times are difficult'; *delo sročnoe* 'the matter is urgent'; *bor'ba upornaja* 'the struggle is bitter'; *pogoda tëplaja* 'the weather is warm'; *noč' tëmnaja* 'the night is dark'; *zima xolodnaja* 'winter is cold'. These are not Class II adjectives since *S* is possible: *noč' temna, bor'ba uporna, pogoda tepla*, etc.

2.3. Certain Class I (and Class III) adjectives are always *S* in fixed idiomatic expressions and proverbs. In these expressions the word-order may be fixed; at any rate it plays no special role. Complements may be present: *gorek čužoj xleb* 'a stranger's bread is bitter'; *gol' na vydumki xitra* 'necessity is the mother of invention'; *neroven čas* 'anything might happen'; *ruki korotki* 'he cannot do anything'; *u straxa glaza veliki* 'through the eye of fear danger looks greater'; *dolg platežom krasen* 'one good turn deserves another'; *on zadnim umom krepok* 'he likes to second-guess'; *on tjažël na pod"ëm* 'he is sluggish'; *ne velika beda* or *beda ne velika* 'there's no harm'; *on vernulsja cel i nevredim* 'he returned safe and sound'; *u menja xlopot polon rot* 'I have a million things to do'; *opjat' streločnik vinovat* 'the little guy always gets it in the neck'; *ljubov' zla—poljubiš' i kozla* 'love is blind'; *legok na pomine!* 'speak of the devil!'; *ne tak strašen čort, kak ego maljujut* 'things aren't as bad as they are painted'; *čem bogaty, tem i rady* 'we'll be happy with what we have'; *on ni živ ni mërtv* 'he is scared to death'.

3. Syntactic Factors

3.1. When a Class I adjective is placed before the subject to achieve stress, a stylistic effect, or both, *S* generally prevails: *slab čelovek!* 'man is weak!'; *xoroša pora urožaja!* 'harvest time is beautiful!'; *tjažela soldatskaja dolja!* 'a soldier's lot is hard!'; *krasivy tam devuški* 'the girls are pretty there'; *krasnorečiva zapis' gospodina Ivanova* 'Mr. Ivanov's record is eloquent'; *ljubopytna istorija vozniknovenija sada* 'the story of how the garden came into being is curious'; *xoroši večera osen'ju* 'the autumn evenings are beautiful'. Occasionally, however, *L* is anteposed: *strannyj ty!* 'you are strange!'; *umnyj ètot čelovek* 'this fellow is clever'.

3.2. When a complement is present, the situation varies with the Class.

3.2.1. *S* prevails for Class I and is always used for Class III: *èti otkrytija važny dlja vsex* 'these discoveries are important for everyone'; *on mstitelen po nature* 'he is vengeful by nature'; *mašina èkonomična v rasxodovanii gorjučego* 'the engine is economical in fuel consumption'; *vaša strana bogata neft'ju* 'your country is rich in oil'; *moi kollegi ravnodušny k ètoj istorii* 'my colleagues are indifferent to this story'; *kompanii svobodny ot vsjakogo kontrolja* 'the companies are free of any control'; *sputnik raven zvezde pjatoj veličiny* 'the satellite is equal to a star of the fifth magnitude'; *naša strana čužda bol'šinstvu sovetskix graždan* 'our country is alien to the majority of Soviet citizens'; *my dovol'ny komnatoj* 'we are satisfied with the room'; *on poxož na tebja* 'he looks like you'; *studenty gotovy k èkzaminam* 'the students are ready for the exams'.

3.2.1.1. *L* for Class I is encountered infrequently: *on ko vsem dobryj* 'he is kind to everyone'; *rajon dlja turistov neinteresnyj* 'the region does not interest tourists'.

3.2.2. Class II is *L*: *ruki koričnevye ot zagara* 'his hands are brown from the sun'; *èta komnata podxodjaščaja dlja menja* 'this room is suitable for me'.

3.3. When the adjective is the auxiliary of an infinitive, Classes I and III are encountered as such auxiliaries. They are always *S*: *Saša sposoben rabotat'* 'Sasha is capable of working' (cf. *on sposobnyj* (or *sposoben*) 'he is capable'); *ne budet li on ljubezen èto objasnit'?* 'will he be so kind as to explain this?'; *ja sčastliv ob ètom uznat'* 'I am happy to learn of this'; *deti dolžny otdyxat'* 'children must rest'; *S.S.S.R. ne nameren ispol'zovat' ètot zakon* 'the U.S.S.R. does not intend to use this law'; *Gorja gorazd torgovat'sja* 'Gorja is good at bargaining'; *gosti gotovy uexat'* 'the guests are ready to leave'; *Petrov prizvan sledit' za sobljudeniem dogovora* 'it's Petrov's mission to see that the treaty is carried out'.

3.4. When the subject is a neuter pronoun, *S* prevails for Class I and is always used for Class III: *èto važno* 'this is important'; *vsë jasno* 'everything is clear'; *èto interesno* 'this is interesting'; *vsë gotovo* 'everything is ready'; *odno nužno* 'one thing is necessary'; *interesno to, čto on ne prišël* 'it's interesting that he didn't come'. Class II occurs rarely with neuter pronouns. It is regularly *L*: *vsë koričnevoe* 'everything is brown'.

3.5. Constructions of the type *u menja* occurred in the corpus mostly with Classes I and II.

3.5.1. *L* predominates for Class I: *poxodka u nego pravil'naja* 'his gait is correct'; *stixi u neë ploxie* 'her poetry is bad'; *nastroenie u vsex bodroe,*

pripodnjatoe 'everyone is in a cheerful, excited mood'; *sovest' u Il'ina nesposobnaja k dlitel'nomu soprotivleniju* 'Ilyin's conscience is not capable of prolonged resistance'; *den' u menja svobodnyj* 'I am free today'; *narod u nas xorošij* 'our people are good'; *pišča u nix vsegda gorjačaja* 'they always have hot food'.

3.5.1.1. *S* usage for Class I is infrequent and may appear bookish: *lico u nego bledno* 'his face is pale'; *jabloki u vas xoroši* 'your apples are good'; *glaza u neë svezi* 'her eyes are clear'.

3.5.2. Class II is *L*: *dom u vas bol'šoj* 'you have a large house'; *pal'to u nego koričnevoe* 'he has a brown coat'; *jazyk u nix russkij* 'their language is Russian'; *akcent u nego nastojaščij* 'his accent is real'.

3.5.3. Class III is usually *S*: *ruki u nego vymyty* 'his hands are washed'; *vopros u nix razrabotan* 'they have the question worked out'.

4. Effect of the Copulative Verb

4.1. After *byl–budet* 'was'–'will be', i.e., the past or future of *byt'*, usage varies with the Class.

4.1.1. For Classes I and II, *I* predominates: *ego doklad byl obstojatel'nym* 'his report was detailed'; *vyzdorovlenie ne budet lëgkim* 'the recovery will not be easy'; *programma byla neobyčnoj* 'the program was unusual'; *peregovory budut besplodnymi* 'negotiations will be fruitless'; *nastroenie u nas bylo tem bolee pripodnjatym, čto on tam* 'our mood was all the more enthusiastic since he was there'; *vsë budet poleznym* 'everything will be useful'; *to, čto proizošlo, bylo neožidannym dlja menja* 'I had not expected this to happen'; *ètot sotrudnik byl takim že nenadëžnym, kak i vse predyduščie* 'this worker was as unreliable as all his predecessors'; *trudnymi byli uslovija* 'conditions were difficult'; *pogoda byla tëploj* 'the weather was warm'; *ego vystuplenie bylo lučšim* 'his performance was the best'.

4.1.1.1. *L* and *S* occur less frequently: *on byl očen' umnyj* 'he was very clever'; *pogoda byla xolodnaja* 'the weather was cold'; *upakovka byla ploxaja* 'the packing was bad'; *dom byl bol'šoj* 'the house was large'; *starik byl očen' privetliv* 'the old man was very friendly'; *zadača budet interesna* 'the problem will be interesting'.

4.1.1.2. In certain instances there may be a semantic difference between *I* or *S* and *L*.[14] *I* or *S* can sometimes suggest a temporary quality, whereas *L* can point to a permanent quality or can attribute a quality to the subject

[14] Cf. Academy, *Grammatika*, II, 462 and 466.

with no implication at all as to permanency or non-permanency. Examples: *vo vtoroj časti koncerta skripač byl menee ubeditel'nym* 'during the second part of the concert the violinist was less convincing'. The time modifier in this sentence underscores the temporary nature of the quality described by the adjective. The following example is similar: *v tečenie goda rabota byla trudnoj* 'the work was difficult during the year'. On the other hand, in the next example no implication as to permanency is made: *stixi byli ploxie* 'the poetry was bad'. *I* in this sentence could imply 'at that time', 'in that spot', etc. *L* in *dom budet bol'šoj* implies 'the house (which they will find) will be big', whereas *I* (*dom budet bol'šim*) suggests 'the house (which they will build). . .'. Likewise, the sentence *on byl p'janym* (or *p'jan*) could suggest 'he was drunk then', whereas *on byl p'janyj* suggests 'he was a drunk'.

However, in modern Russian this distinction is often not observed. The great increase in *I* usage with *byl–budet* in the last hundred years[15] has tended to obscure any semantic differentiation. Thus, in contemporary Russian it is quite possible to say *steny byli tonkimi* 'the walls were thin' with *I*, even though a permanent quality is manifest. In several of the examples cited in 4.1.1 there is no connotation of a temporary quality.

4.1.2. Class III remains *S*: *my budem dovol'ny* 'we will be satisfied'; *sputnik byl viden* 'the satellite was visible'; *on byl vinovat* 'he was guilty'; *vzryv budet slyšen* 'the explosion will be heard'; *on dolžen byl uexat'* 'he had to leave'; *nužny byli den'gi* 'money was needed'; *avtobus budet gotov* 'the bus will be ready'; *otec byl prav* 'father was right'; *mir byl by obespečen* 'peace would be assured'; *spasatel'nye raboty byli zaveršeny* 'rescue work was completed'.

4.2. After the infinitive *byt'*, usage also varies with the Class.

4.2.1. When no finite verb or *S* adjective (see 3.3) serves as the auxiliary of *byt'*, *I* is used for all classes: *u nas est' osnovanija byt' nedoverčivymi* 'we have reasons to be incredulous'; *čelovek s talantom možet sebe pozvolit' byt' skromnym* 'a man with talent can afford to be modest'; *seržant poprosil razrešenija byt' svobodnym* 'the sergeant asked for permission to be dismissed'; *nužno byt' bol'šim, čtoby èto sdelat'* 'you have to be tall in order to do that'; *on stojal ostorožno, čtoby byt' gotovym k otstupleniju* 'he stood cautiously so as to be ready for a withdrawal'; *on prigibalsja, čtoby ne byt' zamečennym vragom* 'he stooped down so as not to be spotted by the

[15] In all of Pushkin's prose, *I* was used with *byl–budet* only four times. See Benson, 163.

enemy'; *zalog našej nezavisimosti podvergaetsja opasnosti byt' prodannym* 'the guarantee of our independence is in danger of being sold out'.

4.2.2. When a finite verb or *S* adjective (see 3.3) serves as the auxiliary of *byt'*, usage seems to be as follows:

4.2.2.1. For Classes I and II, *I* prevails: *ja xoču byt' otkrovennym s toboj* 'I want to be frank with you'; *Ol'ga ne mogla byt' vesëloj* 'Olga couldn't be gay'; *vrač staraetsja byt' vežlivym* 'the doctor is trying to be polite'; *koncert obeščaet byt' zanimatel'nym* 'the concert promises to be entertaining'; *èti gruppy mogut byt' poleznymi dlja dela mira* 'these groups can be useful for the cause of peace'; *naši divizii dolžny byt' peredovymi* 'our divisions must be first-rate'.

4.2.2.2. Although *I* does dominate for Class I, the corpus reveals that *S* can also be encountered: *my dolžny byt' ostorožny* 'we must be careful'; *on xočet byt' ljubezen* 'he wants to be nice'; *kniga možet byt' polezna dlja tex, kto izučaet ispanskij jazyk* 'the book can be useful for those who are studying Spanish'. The use of *L* is rare: *ona staraetsja byt' vesëlaja* 'she is trying to be gay'.

4.2.2.3. For Class III the corpus showed a prevalence of *S*: *otsjuda gorod ne mog byt' viden* 'from here the city could not be seen'; *naši sojuzniki dolžny byt' gotovy* 'our allies must be ready'; *malye strany mogut byt' polnost'ju uničtoženy* 'small countries can be wiped out completely'; *mir možet byt' brošen v vojnu* 'the world can be thrown into war'. *I* is sometimes encountered: *on dolžen byt' gotovym* 'he must be ready'.

4.2.2.4. *L* may occasionally occur for Classes I and II: *mesto dolžno byt' spokojnoe* 'it has to be a quiet spot'; *zvuk možet byt' gluxoj* 'the sound may be unvoiced'.

4.3. After the imperative *bud'(te)* 'be!', Classes I and III were encountered. They were always *S*: *bud'te ljubezny, ostorožny, gotovy* 'be nice, careful, ready'. In the following example the adjective after *bud'te* serves as the auxiliary of an infinitive: *bud'te dobry zapolnit' anketu* 'be so kind as to fill out the questionnaire'.

4.4. A separate statement may be made for cases when the copulative verb is one that usually serves as an independent predicate in an actor–action construction. (In most instances, the verb denotes motion or position—*xodit'* 'go', *sidet'* 'sit', *idti* 'go', *prixodit'* 'come', *priezžat'* 'come', *vozvraščat'sja* 'return', etc.).

4.4.1. When predicate adjectives appeared with these verbs they were chiefly Class I or III. *L* and *I* are apparently free variants for any Class

when the verb is finite. No semantic differentiation could be established. Examples of *L*: *Kolja prišël trezvyj* 'Kolja came sober'; *ja šël p'janyj* 'I walked along drunk'; *Volodja vstal blednyj i mračnyj* 'Volodja got up pale and sullen'; *Ivan vyšel iz kabineta ozabočennyj, ustalyj* 'Ivan left the office worried and tired'; *predloženie vernulos' odobrennoe i rekomendovannoe k vnedreniju* 'the proposal came back approved with a recommendation to put it into effect'; *on sošël gotovyj k boju* 'he came down ready for a fight'; *on lëg spat' dovol'nyj soboj* 'he went to bed satisfied with himself'; *ženščina sidela okružënnaja det'mi* 'the woman sat surrounded by children'. Examples of *I*: *Paša vernulsja k sebe zadumčivym* 'Pasha returned home wrapped in thought'; *boec upal mërtvym* 'the soldier fell dead'; *pis'mo vyšlo suxim* 'the letter came out uninspiring'; *ja vozvraščajus' bodrym* 'I am returning in good spirits'; *on prišël pervym* 'he came first'; *naše pravitel'stvo vyšlo iz vojny eščë bolee okrepšim* 'our government emerged from the war stronger than ever'; *on sgorel živym* 'he was burnt alive'; *on vošël gotovym k puti* 'he came in ready to leave'; *ego prebyvanie v gorode prošlo nezamečennym* 'his stay in town passed unnoticed'; *Boris uxodit domoj razbitym* 'Boris is leaving for home crushed'.

4.4.2. After infinitives *I* prevails: *nel'zja xodit' mokrym* 'you can't walk around wet'; *on uspel vyjti suxim iz vody* 'he managed to get off unpunished'; *on ne xotel vernut'sja utomlënnym* 'he did not want to return all tired out'; *spektakl' obeščal vyjti zanimatel'nym* 'the show promised to be entertaining'.

4.4.3. A past passive participle may serve as the copulative verbal form: *most byl brošen nevzorvannym* 'the bridge was abandoned before it was blown up'; *čelovek pokazan prinižennym, bespomoščnym* 'man is shown humiliated, helpless'.

4.4.4. If a conjunction such as *kak* 'as', 'like', *točno* 'as' stands before the predicate adjective, *L* is used: *lošad' stojala kak vkopannaja* 'the horse stood as if glued to the spot'; *oni xodjat kak p'janye* 'they walk around as if drunk'; *ona stojala točno udivlënnaja* 'she stood as if surprised'.

4.4.5. The use of *S* with verbs of independent predication in contemporary Russian is rare and may have a bookish, obsolescent coloring: *on sidel bleden* 'he sat pale'; *djadja vošël surov* 'uncle came in looking stern'.

4.5. We come now to verbs such as *stanovit'sja* 'become', *kazat'sja* 'appear', *okazyvat'sja* 'turn out', *ostavat'sja* 'remain', *delat'sja* 'become', *byvat'* 'be habitually', etc.

4.5.1. *I* prevails for Classes I and II: *organizacii stali aktivnymi* 'the organizations became active'; *èto kažetsja verojatnym* 'this seems probable'; *oni okazalis' bessil'nymi* 'they turned out to be powerless'; *oni ostajutsja vernymi principam respubliki* 'they remain true to the principles of the republic'; *on delaetsja skupym* 'he's becoming stingy'; *uslovija redko byvajut blagoprijatnymi* 'conditions are rarely favorable'; *xozjajstvo stalo bol'šim* 'the farm became large'.

4.5.1.1. *S* is not frequent for Class I, but it does occur. Sometimes it is suggestive of a literary style: *rabotniki ostalis' ravnodušny k ètoj istorii* 'the workers remained indifferent to this story'; *slëzy kažutsja iskrenni* 'the tears appear genuine'; *on sdelalsja razgovorčiv* 'he became talkative'; *èto pokažetsja stranno* 'it will seem strange'. *L* occurs rarely: *ona stala strogaja* 'she became strict'.

4.5.2. For Class III, *S* prevails: *on okazalsja prav* 'he turned out to be right'; *stali vidny rezul'taty* 'results became visible'; *passažiry ostalis' dovol'ny poezdkoj* 'the passengers were satisfied with the trip'; *on stal poxož na mal'čika* 'he became like a boy'. *I* also occurs, especially for past passive participles: *gorod kažetsja poxožim na Tbilisi* 'the city looks like Tiflis'; *partija ostalas' nezakončennoj* 'the game remained unfinished'; *dver' okazalas' otkrytoj* 'the door happened to be open'.

4.5.3. With infinitive forms of the verbs in question, *I* predominates for all Classes: *on sumel sdelat'sja populjarnym* 'he managed to make himself popular'; *on xočet kazat'sja bol'šim* 'he wants to appear tall'; *nel'zja okazat'sja otrezannym* 'you mustn't be cut off'. The use of *S* is rare and did not occur in the corpus. In the following example cited by Šaxmatov,[16] the use of *S* falls under section 3.1: *kak durna dolžna ja kazat'sja!* 'how wicked I must seem!'.

4.5.4. The corpus indicated that certain verbs of the general type under discussion are ALWAYS used with *I* regardless of Class. Such verbs are *vygljadet'* 'look', 'appear', *čuvstvovat' sebja* 'feel', *pritvorjat'sja* 'pretend to be', *slyt'* 'be known as', etc. Examples: *deti vygljadjat zdorovymi* 'the children look healthy'; *on čuvstvuet sebja ustalym* 'he feels tired'; *on pritvorjaetsja ranenym* 'he pretends to be wounded'; *ona slyvët umnoj* 'they say she's clever'.

5. COMMENT

This study has demonstrated the complexity of predicate adjective usage in modern Standard Russian. This complexity is aggravated by diachronic

[16] *Op. cit.*, 352.

considerations—the increase of *L* and *I* usage. The existence of such an involved grammatical structure and free variants led to confusion at times among the native informants. The following statement by André Mazon is a fitting conclusion to this paper: "Il semble parfois que la langue dispose d'un système de moyens d'expressions trop abondant et qui dépasse la subtilité de l'homme moyen"[17] (*op. cit.*, p. 391).

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Semantic Components of Pronoun Systems:

Maranao

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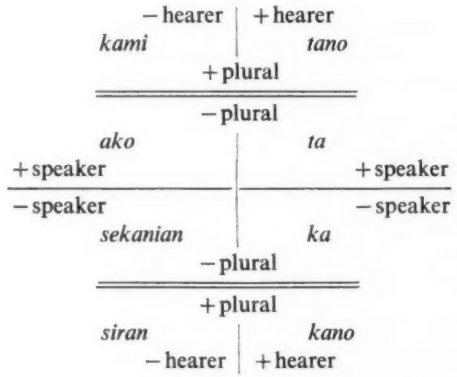
The following translations indicate the persons expressed by Maranao[1] pronouns (spoken by some 250,000 persons in Lanao, Philippines): *ako* 'I', *ka* 'thou', *sekanian* 'he, she, it', *kami* 'we-not-you', *tano* 'we-all', *ta* 'we-two', *kano* 'you', and *siran* 'they'. These pronouns are chosen as representative since there are four different sets, each set indicating different grammatical relations but the same persons. The semantic content of these pronouns may be indicated by showing the presence or absence of the components *speaker*, *hearer*, and *plurality*. The following chart indicates the semantic content of each pronoun.



It will be noted that *ta* 'we-two' indicates the speaker and hearer as both present, but in this system is minus plurality since both speaker and hearer are single individuals.

[1] Cf. H. P. McKaughan, *The Inflection and Syntax of Maranao Verbs*, Publications of the Institute of National Language, Manila, 1958.

This analysis differs somewhat from Thomas's for Ilocano pronouns[2] in that he found two types: True Pronouns and Number Pronouns. His suggested semantic components include for the True Pronouns: speaker, hearer, simple, and plus; and for the Number Pronouns: singular and plural. We include in one system even those pronouns that indicate neither speaker nor hearer (3rd person) since for Maranao the differences suggested by Thomas for Ilocano do not occur.

[2] *Word* XI (1955), 204–208.

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Gilyak

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In traditional terms, Gilyak* has forms for personal pronouns in three persons and two numbers. In the first person plural there is a bifurcation, traditionally characterized as "inclusive" vs. "exclusive."

SINGULAR	PLURAL	
ñi 'I'	*miřn* 'we'	Incl.
	ñin	Excl.
ci 'thou'	*cin* 'ye'	
jaŋ 'he, she'	*iřn* 'they'	

This paper is an attempt to present (i) a deeper semantic analysis than

* All the material in this paper was gathered in Abasiri, Japan, during 1956, under a grant from the Ford Foundation for which grateful acknowledgment is made here.

The transcription used here is identical with that used in "Gilyak Nursery Words" (abbreviated GNW), *Word* XII (1956), pp. 260–279, with the following single exception: where, in the previous paper, a hyphen (-) was used, a double hyphen (--) is used now; where, in the previous paper, an equal sign (=) was used, a hyphen is used now (GNW, 264). All the forms cited here, unless otherwise noted, are from the South-Sahalin dialect and were collected from the informant mentioned in GNW.

the traditional, (ii) an analysis of the morphemes of the pronominal forms, and (iii) the historical reconstruction of these forms.[1]

We begin by presenting some morphological, syntactic, and relevant ethnographic information.

(1) The morpheme of the second and third persons singular in the non-finite conjugation of the verb (the "converb") is *-ř*; the morpheme for all other persons, singular or plural, is *-t* (and *-n*). There is thus only partial symmetry between personal pronouns on the one hand and verbal desinences on the other, i.e., we find syncretism in the latter. This is the highest type of complexity in the category of person found in the verbal paradigm. The most common forms of the verb, aorist and present, have no personal forms and are *-nt* throughout. Examples:

ye-t wi-nt 'taking it I (we, ye, they) went = I (etc.) took it and went'
ye-r wi-nt 'you (sg.)/he, she took it and went'
ye-nt '(to) take it'; any person, aorist
ye-j-nt '(to) take it'; any person, present.

(2) "Clipped" forms of the three personal pronouns of the singular (*ñi, ci, jaŋ*), but not of those of the plural, enter into higher complexes with both verbs and nouns which immediately follow them ("incorporation"). The complexes which result in this way form single stress-groups. Pronominal element (= "clipped personal pronoun") plus verb yields an object relationship; pronominal element plus noun yields a possessive relationship. As against these, full pronoun plus verb yields a subject relationship, and full pronoun plus nouns yields an appositive relationship. The latter is rare. Examples:

ñi zu-nt 'I wash (something, it)'; two stress-groups
ñ--zu-nt '(someone) washes me'; one stress-group
ci ñ--zu-nt 'thou washest me'

ñ--ycx 'my old man'
ñi ycx 'I, the old man'; two stress-groups
ñi--fry 'my shelter' (*přy* 'shelter'); one stress-group.

(3) In addition to the three conventional persons, there is a pronoun, *pi* (clipped forms: *p--, pe--, pi--*) which may be glossed as 'one's own, -self (and plural)'. In combination with verbs and nouns it forms complexes and syntagms of exactly the same kind as the pronouns *ñi, ci, jaŋ*. Borrowing from Eskimo terminology, we may call it the FOURTH PERSON. Examples:

p--su-nt '(someone) is washing himself; is bathing'
ñi p--su-nt 'I am bathing'
pi ycx 'the old man himself' (two stress-groups; rare)
p--ycx 'my/your/his/our/their/one's old man'.

(4) In normal discourse and in the recitation of folk-tales personal pronouns are generally not used. The singular is more frequently found than the plural and the first and second persons are more common than the third. Clipped forms, on the other hand,

[1] All previous works on Gilyak grammar merely list the pronouns, with little or no comment on their form and function. These works are listed in R. Austerlitz, "Vocatif et impératif en Ghiliak" (abbr. VIG), *Orbis* VII (1958), p. 481. See also R. Jakobson, G. Hüttl-Worth and J. F. Beebe, *Paleosiberian Peoples and Languages: A Bibliographical Guide*, New Haven: HRAF Press, 1957.

are extremely frequent, both in nominal complexes (*c--ycx* 'thy old man') and in verbal ones (*c--su-nt* '[someone] bathes you').

(5) In conversation, kinship terms or words such as "old man, neighbor, friend, guest" and the like very frequently replace the pronoun *ci* 'thou' which carries a certain stigma of impoliteness. There is also an obsolescent practice of substituting *cin* 'ye' for *ci* when addressing a single person.[2]

(6) The form for the third person singular, *jaŋ*, is used by newly-weds to address each other. In the culture, this is interpreted as a sign of timidity.

(7) There is a trace of a reciprocal pronoun, **w--*, which can be reconstructed historically but which, synchronically can be localized only in a series of morphophonemic alternations. It will not be discussed here.

I. SEMANTIC ANALYSIS. While there is nothing incorrect in the traditional notions such as number and person to characterize personal pronouns, the categories employed are generally vague and not exhaustive. In this analysis we tentatively propose the three principal notions: FOCUS, PRESENT, and EGO.

FOCUS is used to indicate that the deixis is directed at a single point. Thus, *ci* 'thou' is a focus and *cin* 'ye' is more than, but containing, that focus. As the FOCUS is relaxed we pass to SPECTRUM.

PRESENT is to be taken in both its temporal and spatial sense and indicates that the act of speech overtly refers to the here and now. Thus, *ci zu-nt* 'thou washest or thou didst wash (something)' by necessity confronts the verb, regardless of its tense or aspect, with the present, since *ci* is present at the time of the utterance. The same is true of *ñi* and the combinations into which it enters. But the same would not necessarily be true of any statement about *jaŋ* 'he, she', since *jaŋ* is not necessarily present at the time of the utterance.

EGO indicates the origin of the speech event and, as such, is a further restriction of a PRESENT FOCUS. In the case of a SPECTRUM of EGO, however, we may deal with one which is PRESENT as well as with one which is not PRESENT.[3]

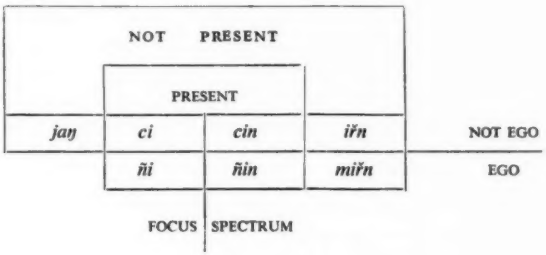
Application of the three above notions and their counterparts will permit us to avoid such designations as "first person plural" which,

[2] Cf. T. Hattori, "Giriyâkugo no keigoteki hyôgen ni tuite" (Honorifics in Gilyak), *Minzokugaku kenkyû*, N.S., Vol. 1, No. 5, Tokyo, June 1943, pp. 9–16. For polite vs. less polite forms in the imperative and vocative, see VIG, 477–500.

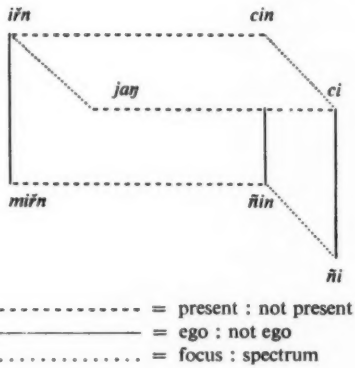
[3] It will be seen that each of these three notions has its counterpart. Each counterpart may be considered as a relaxation of the departing notion. In this way, we are dealing with three sets of two notions: *focus* : *spectrum*, *present* : *not present*, and *ego* : *not ego*. By re-applying the pair *focus* : *spectrum* to the pair *present* : *not present* (i.e., *focus-of-present* : *relaxation of focus-of-present*) we may forego the establishment of the categories *ego* : *not ego* and thus emerge with only two departing notions, *focus* and *present*. The question whether such a reduction is warranted or useful is open.

strictly speaking, would indicate that the speakers are making a statement in unison. ("First person plural," in traditional terms, may mean: speaker +speaker [?], speaker+non-speaker; non-speaker may mean addressee or non-addressee. Some distinctions not made in these terms will be seen to be crucial in the case of Gilyak.) Graphically, we may choose one of two representations:

(a)



(b)



The traditional labels "inclusive vs. exclusive" by means of which the distinction between *miřn* and *ñin* was formerly characterized can now be defined "not present spectrum of ego" and "present spectrum of ego" respectively. It will be noted that the oppositions *cin* : *iřn* and *ci* : *jaŋ* reflect the same relation as the opposition *ñin* : *miřn*.

The most frequent occurrence of *miřn* is in the formula *miřn ŋafq* 'our friend' which refers to the hero of epics and folk-tales. Reference to such a hero justifies a form different from a PRESENT spectrum of ego.

The difference in degree of politeness between *ci* and *cin* mentioned above

(5) can now be re-interpreted as the difference between focus and spectrum. The element of politeness is thus localized in relaxing the deixis. Similarly, the substitution of *jaŋ* for *ci* (6, above) entails transference from the present to the non-present sphere ("timidity").

In the conjugation of the converb, the forms for the focus of non-ego require one desinential morpheme; all the other forms require another (cf. 1, above).

The missing eighth form in the two above diagrams indicates that there is no form for a "non-present focus of ego" which would presuppose a speaker who is not here now using an ego form, a kind of Biblical burning bush.

II. Formal Analysis. Even the brief semantic analysis above revealed the obvious parallelism

ñi : *ñin*
ci : *cin*.

The final *n* which can be thus isolated and which also appears in *iřn* and *miřn* recurs in the collective ("plural") morpheme *-kun/-xun* and in the correlative *-kin/-xin* '(both . . .) and'. Generally speaking, the phoneme *n* is rare in free morphemes and frequent in bound morphemes.

The various allomorphs of the clipped forms give a complete picture of the forms *ñi*, *ci*, and *jaŋ*. (The plural has no clipped forms.) For the sake of completeness, the allomorphs of *pi* '-self' are included in the table.

pi	*jaŋ*	*ci*	*ñi*	Followed by stem beginning with:
pi--	*ji--*	*ci--*	*ñi--*	cluster plus high vowel (*i, y, u*)
pe--	*je--*	*ce--*	*ñe--*	cluster plus low vowel (*e, a, o*)
		t--	*n--*	single dental (*t-, d-, ř-, r-, n-, l-*)
p--	*j--*			
		c--	*ñ--*	any other single phoneme, including vowel

Examples:[4] *ji--řly* 'his sky' (*tly* 'sky'); *je--řla* 'his harpoon' (*tla* 'harpoon'); *p--wo* 'one's village'; *t--řu* 'thy sleigh' (*tu* 'sleigh'), *c--xu* 'thy arrow' (*ku* 'arrow'), *n--raf* 'my house' (*daf* 'house'), *ñ--val* 'my mountain' (*bal* 'mountain'); *ñ--al* 'my skin' (*hal* 'human skin'), *ñ--al* 'the inside of my mouth' (*al* 'buccal cavity').

[4] Morphophonemic changes in the initial consonants of the stems which follow the clipped forms are not discussed here. See GNW, 263 and the over-all treatment in R. Jakobson, "Notes on Gilyak," *Studies Presented to Yuen Ren Chao on his Sixty-fifth Birthday* (= *The Bulletin of the Institute of History and Philology*, Academia Sinica, vol. 29), Taipei, 1957, pp. 255–281, especially p. 268.

The pronominal forms *ñi, ci, jaŋ* undergo similar changes when followed by a special form of the suffix *-doχ/-roχ* 'to, toward':

ñe-rχ, ce-rχ, e-rχ.

We may sum up the information contained in the above examples by saying that: (1) The underlying forms for the first and second persons singular (present focus) are palatal (*ñ, c*), the dental variants (*n, t*) being conditioned. (2) The characteristic of the first person is nasality (*ñ, n*) and that of the second, clusility (*c, t*). (3) The vowel in these forms is *i* in the case of the free pronominal form and *i, e,* or zero in the clipped forms. (4) The clipped correlates of the free form *jaŋ* are, similarly, *ji--, je--* and *j--*; there is another variant, *e--*.[5] (5) All the plural (spectrum) forms contain *n* in final position. (6) The forms *iřn* and *miřn* cannot be further analyzed synchronically.

III. RECONSTRUCTION. Comparison of the Gilyak dialects as well as information from within the dialect treated here permit us to assume that the five series of consonants of present-day Gilyak (fortis stops *p, t, c, k, q* and corresponding lenis stops and fortis and lenis fricatives) can be historically reduced to three series (which we shall write **P, *T, *K*).[6] Thus, the alternations *t* : *c* and *n* : *ñ* mentioned in the above formal analysis are, historically, traces of the earlier consonantism in which the palatal and dental spheres were not phonemically distinct. There is also other evidence: (1) There are very few words with the initial sequences *ti* and *di*. This would indicate that the sequences *ci* and *ʒi* (of which there are many), originally variants of the sequences **Ti* and **Di*, have now phonemically replaced the sequence "dental plus *i*." The alternation *ř* : *s* can be observed, though rarely, within the dialect, viz., *takř* = *taks* 'Ledum palustre (a tree)'. The same alternation is very widespread interdialectally, in both stems and suffixes. (3) Clusters of the type "dental plus palatal" or "palatal plus dental" do not occur in morpheme-initial position, e.g., *tlaŋi* 'reindeer' which is *colŋi* in the continental dialect.

We may therefore reconstruct *ñi* and *ci* as **NV* and **TV* (where **N* = non-labial and non-velar nasal, **T* = non-labial and non-velar fortis stop, and **V* = front [i.e., non-labial and non-velar] vowel, *i* and *e*).

[5] The pre-verbal (but not pre-nominal) clipped forms *i--, e--* and *ja--* also occur on occasion. Since they are irregular and do not affect our argument they will not be discussed here.

[6] The phonemic system of this dialect is described and discussed in GNW, 261ff. See also R. Jakobson, "Notes on Gilyak," and E. A. Krejnovič, *Fonetika nivxskogo jazyka*, Moscow–Leningrad, 1937.

Similarly, we have reason to believe that *j* developed from *i* (and/or *e*) and *w* from *u* (and/or *o*).[7] Leaving the form *jaŋ* aside for the moment, we may trace the forms *j--*, *ji--*, *je--* and *e--* to ***V***.

It has been said that the form *jaŋ* is textually the least frequent.[8] This may indicate that it is historically the most recent. Its final *ŋ* is the nominalizing or pre-nominalizing element peculiar to our dialect which occurs in nouns (e.g., *ñiɣvŋ* 'man, Gilyak': continental dialect *ñifx*; *jupkaŋ* 'skirt' < Russian [júpka]; *lavŋ* 'Olcha (a tribe)' < *la* 'the River Amur' + *fi-nt* 'is in a place') and verbs (e.g., *hajm-ŋ kuɣi* = *hajm-la-ŋ kuɣi* 'an old Ainu', *hajm-nt* 'is old'). On rarer occasions it also occurs as a phrase-nominalizer: *du wo fi-ŋ* 'those in this village' (*du* 'this, these'). There is nothing surprising about the presence of a nominalizing morpheme in a pronominal form which, as the facts suggest, may be of recent origin. How then do we account for the vowel in *jaŋ*?

We may only conjecture that the form *jaŋ* is somehow related to the interrogative verb *jan-nt* 'how?, why?' which most commonly appears in the following forms: *jan-lax* 'how, why? (lit. "how-because?")', *jan-ř* 'how?', *jan-n-doχ* 'to what end?', *jan-i-ny-nt* 'how (from now on)?'. Whatever the connection between the pronoun and this verb, experience with other languages shows that the third person (non-present non-ego) often borders on or merges with demonstrative or interrogative systems.

The final *n* in *ñin*, *cin*, *iřn*, *miřn* was connected with the "plural" morpheme *-kun*/*-xun* above. It is unlikely that this *n* is connected with the so-called "possessive pronouns"

ñi-ni 'my thing, mine'	*ji-ni* 'his thing'
ci-ni 'thy thing, thine'	*pi-ni* 'one's own'

and their corresponding plurals

ñin ny-nt, *miřn ny-nt* 'ours'
cin ny-nt 'yours'
iřn ny-nt 'theirs'.

(The verb *ny-nt* is a vulgar form of 'he does' and also appears nominally

[7] Cf. R. Jakobson, "Notes on Gilyak," p. 272.

[8] M. Takahasi, *Minami Karahuto giriyakugo* (*The Gilyak Language of South Sahalin*), Osaka, 1942, p. 36 does not even list it but gives (in our transcription) *hu ñiɣvŋ* 'that man' which is, indeed, more frequently used than *jaŋ*. E. A. Krejnovič, "Nivxskij (giljackij) jazyk," in *Jazyki i pis'mennost' paleoazijatskix narodov*, Moscow–Leningrad, 1934, p. 204, gives *if* 'he' (continental dialect) in which we analyze the *-f* as a local-temporal element found in nouns and verbs. (See also R. Austerlitz, "Gilyak Religious Terminology in the Light of Linguistic Analysis," forthcoming in the *Transactions of the Asiatic Society of Japan* for 1957.)

as 'thing'.) The morpheme *-ni* only appears under the conditions cited here.

Unlike the parallelism in *ci* : *cin* and *ñi* : *ñin*, the pair *jaŋ* : *iřn* has nothing more in common than the element **V* (*j*, *i*). The *ř* in *iřn* will remain unexplained.[9]

Finally, the form *miřn*, so obviously reminiscent of *iřn* 'they' both formally and in terms of the semantic analysis, suggests the possibility that its initial *m* derives historically from the numeral-morpheme **me/mi-* 'two'.[10] Phonically, this is the only morpheme of its kind. There are over twenty classes of numerals of which four will be cited. (With some exceptions, there is syncretism of classes above the numeral 'six'.)

	'1'	'2'	'3'	'4'	'5'
General	*ñeqř*	*meqř*	*ʒaqř*	*nykř*	*toqř*
Humans (*ñiγvŋ*)	*ñenŋ*	*menŋ*	*ʒaqř*	*nyrŋ*	*torŋ*
Sleighs (*tu*)	*ñiř*	*meř*	*ʒeř*	*nyř*	*toř*
Canoes (*mu*)	*ñim*	*mim*	*ʒem*	*nym*	*tom*

If the assumption is admissible that the *m* in *miřn* indicates duality, we may say that this reference to duality serves to point to the participation of ego in *iřn*.

The reconstructed forms may be schematically represented as follows:

jaŋ < **V-a-ŋ*	*ci* < **TV*	*cin* < **TV-n*	*iřn* < **V-ʔř-n*
	ñi < **NV*	*ñin* < **NV-n*	*miřn* < **m-iřn*

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[9] Both *-ƒ* and *-ř* (agent, converb) are noun-forming suffixes. We could establish some kind of *artificial* parallelism by considering *if* 'he' (continental form, see note 8) as < **i-ƒ*(**V-ƒ*) and *iřn* as < **V-ř-n*. But Krejnovič (*loc. cit.*) gives *imŋ* for the continental dialect as corresponding to our *iřn*.

[10] M. Takahasi, *op. cit.*, p. 36, cites a form "*myzn*" (=our *miřn*) of which he says (*ibid.*, footnote) that it is not used independently, adding: "Rarely, the dual *merŋ*, *meŋ* 'we two (human beings)' is used." By confusing the numeral "two," class of human beings, with the form *miřn*, Takahasi came very close to an analysis similar to ours.— E. A. Krejnovič, *op. cit.*, p. 205, cites *mer* 'our' as a "possessive pronoun" and does not include it with the personal pronouns, p. 204. My own informant has used *miřn* pronominally but has never been able to say anything clear about its distinction from *ñin*.

WILLIAM DIVER

Palatal Quality and Vocalic Length in Indo-European

The concept of the existence of so-called laryngeal consonants in the earlier stages of the Indo-European languages is now accepted in principle by at least a task force of Indo-Europeanists,[1] and already some progress has been made in reinterpreting certain phenomena of the individual languages in terms of laryngeal theory. This process is of course mutually beneficial; analysis of irregularities in the various shapes of certain morphological items provides data on which the concept of a lost phoneme is based, whether the phoneme is the Greek *wau* or the Indo-European laryngeal. Once the concept is established it can be used to account for other, less tractable, irregularities. The most striking advance in recent research in this area has been André Martinet's suggestion[2] that the third, the *o*-coloring, laryngeal had as one of the components of its articulation a labio-velar element. This suggestion is particularly attractive not only because of the "regular irregularities" thus explained, but also because the assumption of a labio-velar characteristic gives to a laryngeal for the first time a phonetic feature in common with one of the orders of the traditional Indo-European consonant pattern.

It will be the purpose of this article to present evidence suggesting that one of the phonetic components of the so-called first laryngeal was a palatal articulation.[3] The evidence presented will consist of morphophonemic alternations between length and *y*, and will be drawn from two morphological classes, the denominative suffix **ye/o* and roots in **ē*.

The existence of a denominative suffix **ye/o* in Indo-European has long been recognized and is well attested by such verbs as Sanskrit *deva-yá-ti* 'serve the gods', Greek *philéō* 'love' (< **philé-yō*), Latin *albeō* 'be white'

[1] For an account of the present state of laryngeal studies, see the report by André Martinet for the Eighth International Congress of Linguists (Oslo, 1957): "Les 'Laryngales' indo-européennes".

[2] *Economie des changements phonétique* (Berne, 1955), §8.

[3] It is suggested that the symbol **H*^{y} be used to indicate the palatal laryngeal.

(< **albe-yō*), Gothic *mikil-jan* 'magnify', Old Church Slavic *želě-jo* 'wish', and others. Yet close examination of the morphophonemic variations that denominative verbs undergo reveals that there is a recurring irregularity—an element of length—that seems to need explanation.

The suffix appears most clearly, in spite of the leveling of the *e/o* alternation, in Sanskrit, where it has the form *-yá-*, added to noun stems ending in either a vowel or a consonant. The consonant stems need not concern us, since they have no vowel to be affected by an increment of length, but where the suffix is added to a noun stem ending in a short vowel there is a pervasive, unexplained lengthening of the vowel.[4] Noun stems in short *i* regularly have long *i* in the denominative stem: from *sakhi* 'friend' is derived *sakhīyáti* 'seek friendship'. Stems in short *u* regularly have long *u* in the denominative: from *śatru* 'enemy' is derived *śatrūyáti* 'be hostile'. In the single instance of derivation from a stem in short *ṛ*, the denominative stem has *rī*, the regular long correspondent of *ṛ*: from *mātṛ* 'mother' is derived *mātrīyáti* 'treat as a mother'. Stems in short *a* are sometimes lengthened, sometimes not: from *aśva* 'horse' is derived *aśvāyáti* 'desire horses', while from *deva* 'god' is derived *devayáti* 'serve the gods'. And already in the Veda the same stem may show fluctuation between a long and a short vowel. In the earlier language the forms with short *a* may well represent an instance of recomposition under the influence of the noun stem, the distinction between long and short *a*-stems thus being preserved in the denominative verb. Whitney points out[5] that this formal distinction is exploited for morphological purposes in the later, classical, language, the long vowel form developing a middle inflection and intransitive meaning, while the short vowel form develops active inflection and transitive meaning. But in spite of this minor detail, which may be considered a morphological disturbance of the regular phonological development, the general pattern is clear. Short vowels are regularly lengthened when followed by the denominative suffix *-yá-*.

Denominatives from short-vowel noun stems in Latin show a similar lengthening of the stem vowel, but here the differences in phonological evolution and morphological reorganization that have given a different appearance to the denominative type as a whole have also made possible the explanation of at least some of the long vowels as the result of a specifically Latin, or Italic, development. The traditional analysis of such forms as *albeō* 'be white', *metuō* 'fear', and *fīniō* 'finish' assumes the addition to the stem of a suffix **ye/o* that has undergone regular phono-

[4] For further examples see William Dwight Whitney, *A Sanskrit Grammar* (Leipzig, 1888), §§1059–1062 and A. A. Macdonell, *Vedic Grammar* (Strassburg, 1910), §§563–566.

[5] *Sanskrit Grammar*, §1059c.

logical loss of the intervocalic *y* (as in *trēs* 'three' from **treyes*) and in some instances coalescence of the two vowels thus brought together: *albeō* < **albeyō*, second person singular *albēs* < **albees* < **albeyes*; *metuō* < **metuyō*, second person singular *metuis* < **metues* < **metuyes*; *fīniō* < **fīniyō*, second person singular *fīnīs* < **fīniis* < **fīnies* < **fīniyes*. In these forms the suggestion of length from coalescence of two short vowels works very well; even in the case of first person plural *albēmus* (< **albeomos* < **albeyomos*), where phonologically one does not expect assimilation of the sequence *eo* in the face of its retention in *albeō*, an appeal to analogy is plausible enough, for the levelling of a single aberrant form to the model of the *albēs* type is not unreasonable. But even analogy is not everywhere a sufficient explanation for the forms in use as past participles of these verbs. Roland G. Kent[6] gives the usual analysis of the long-vowel participles: "Many verbs with stem in a long vowel keep this vowel in the participle: *curātus, fugātus, implētus, fīnītus, audītus, cupītus* . . .," thus invoking in effect the analogy of the present stem to account for the length of the participle. But for forms such as *metūtus*, where there is no long vowel in the present, he can only say: "Denominative verbs to *-u-* stems lengthened the *-u-* in the participle . . .," implicitly denying any reasonable analogical source for the length of the *ū*. One might of course argue that the *ū* of *metūtus* is long by analogy with the long vowels of the participles of other conjugations (a view rejected by Kent, and many will follow his lead), but this is open to the objection that the short *i* of such forms as *monitus*, subject much more directly to the influence of the long *i* of the very numerous verbs of the type *fīnītus*, is not lengthened. Analogy from one conjugation to another, at least among the participles, does not seem to have had much force.

The assumption of a suffix **ye/o* does not provide a satisfactory explanation of the lengthening of short vowels in the Latin denominatives. An alternative analysis is to associate this Latin phenomenon with the parallel one in Sanskrit and assume that the suffix contains a laryngeal that follows the normal phonological rule of dropping between vowels and, before a consonant, dropping and lengthening the preceding vowel. That this is in fact the regular result in Latin is shown by the behavior of root-final laryngeals:

	**impleH-ō*	**impleH-s*	**impleH-tom*
>	*impleō*	*implēs*	*implētum* 'fill'
	**suH-ō*	**suH-e-s*	**suH-tom*
>	*suō*	*suis*	*sūtum* 'sew'

[6] *The Forms of Latin* (Baltimore, 1946), §420.

	**sciH-ō*	**sciH-s*	**sciH-tom*
>	*sciō*	*scīs*	*scītum* 'know'

If a laryngeal is assumed for the denominative suffix, the various forms are seen to develop with complete phonological regularity, and the entire problem of the formation of denominative verbs in Latin is much simplified.

	**albe-H-ō*	**albe-H-s*	**albe-H-mos*
>	*albeō*	*albēs*	*albēmus* 'be white'

	**fīni-H-ō*	**fīni-H-s*	**fīni-H-tom*
>	*fīniō*	*fīnīs*	*fīnītum* 'finish'

	**metu-H-ō*	**metu-H-es*	**metu-H-tom*
>	*metuō*	*metuis*	*metūtum* 'fear'

As can be seen from the above, the denominative conjugation is normally athematic (some athematic forms have survived in Greek; see below), but the *u*-stems, definitely a minority category, have been drawn into the thematic (the Latin third) conjugation in the general reorganization of the present system.

Inclusion of the forms of the past participle in a consideration of the denominative verb is admittedly a departure from the usual analysis, for it is customary to assume that the present is not relevant to forms outside the present system, as is revealed, for example, in Kent's supplying of a special explanation for the long vowel in the participle. Such an assumption is certainly justified for the many Latin verbs, particularly the primary verbs of the third conjugation, that have retained the forms of the Indo-European aorist, perfect, and verbal adjective in **to* in their non-present categories. On the other hand, it would not be surprising, a priori, to find that the denominative stem, which gives the appearance of being a relatively late development in Indo-European itself and is quite a productive element in the verbal system of more than one of the attested languages, has been used as the fundamental stem of a weak verb and extended to the non-present stems. The denominatives do not have stems of the older Indo-European type, and it is more likely that the denominative stem would have furnished the basis for all forms of the verb directly than that they should have originated in various analogical developments. The reason that this simpler explanation has not been accepted is probably twofold. Firstly, the denominative conjugation in Sanskrit is restricted to the present system. Secondly, and more important, in Greek and Latin it is impossible

to derive the non-present forms with any kind of phonological regularity from a stem ending in the suffix **ye/o*. Of these two difficulties the first clearly cannot be taken too seriously if directly opposing evidence is found in other languages; after all, one does not expect a continuing identity in the morphological characteristics of diverging languages. The second objection is automatically neutralized by the assumption that the denominative suffix is not **ye/o* at all but a laryngeal, which does provide a phonologically regular explanation for the forms of the various stems. For example, it is no longer necessary to find special reasons for the long vowel in such Latin participles as *fīnītum* and *metūtum*; the long vowel here derives as regularly from the suffix as does the long vowel of the present.

In Greek too there is a regular lengthening of short vowels in denominative verbs, but the particular manifestation of the problem is of course somewhat different, again because of variations in the details of phonological and morphological development. Intervocalic **y* is lost, as in Latin, but intervocalic **w* and **s* are also lost, together with the intervocalic laryngeal. A denominative of the type *philéō* 'love' is supposed to be derived from **philéyō* with loss of the intervocalic **y*, but this analysis is based entirely on comparative evidence, for the **y* itself does not appear in the Greek forms. From a purely phonological point of view any one of the four consonants that are regularly lost in intervocalic position—**s*, **w*, **y*, or **H*—could have left the hiatus found in *philéō*, and a choice among them should at least be attempted through an examination of the evidence obtainable from morphophonemic alternations. Such evidence is to be found when stems ending in these consonants are followed alternately by the initial vowel or consonant of a suffix or desinence. Examples, cited prevocalically in the present tense and preconsonantally in the future or aorist, are as follows:

s	*zé-ō*	*zés-sa* 'boil'
w	*plé-ō*	*pleú-somai* 'sail'
y	*ti-nō*[7]	*teí-sō* 'pay'
H	*dé-ō*	*dē-sō* 'bind'

These examples show the regular alternation between prevocalic and preconsonantal positions: for *s*, *e/es*; for *w*, *e/eu*; for *y*, *e/ei*; for laryngeal (in this case, as shown by the *e*-coloring of the vowel, the first laryngeal), *e/ē*. Comparison of these alternations with those of denominative verbs in the same environments will provide us with a phonological basis for the

[7] There does not seem to be a clear case of a present of the type **téō* < **téyō*, but the *iota* in the zero grade form *tinō* indicates the presence of the *y* in the root. The important point here is the preconsonantal form seen in *teísō*.

identification of the lost consonant. In such verbs as *philé-ō* 'love', *dēló-ō* 'show', and *hidrú-ō* 'place' the non-present stems, which contain suffixes beginning with a consonant, regularly show a long vowel, not *es*, *eu*, or *ei*: *philé-ō*, *philḗ-sō*, *ephílē-sa*, *pephílē-ka*; *dēló-ō*, *dēlṓ-sō*, etc.; *hidrú-ō*, *hidrû-sō*, etc. The phonological conclusion to be drawn is that the lost consonant in *philéō* and other denominative verbs is a laryngeal, not *s*, *w*, or *y*.

On the other, that is the morphological, hand, it is usually assumed that in Greek the non-present forms of the verb are not based on the present stem and that the long vowel comes from another source, a situation exactly parallel to that already discussed for Latin. For verbs of types other than the denominative there can be little question of the independence of the various stems, as is clearly shown by such a case as *tugkhánō*, *teúxomai*, *étukhon* 'happen'. Again these variations represent an inheritance from a relatively remote chronological stratum, a process of derivation that was productive in primary verbs at an earlier stage of the language. The denominative forms are not a part of this pattern but are a comparatively late "regular" derivational process. No doubt it would long ago have been recognized that the denominatives have a common base in all stems if it were not for the phonological impossibility, here as in Latin, of basing the long-vowel forms on a stem ending in *y*; if the oblique forms of *philéō* were actually **phileísō*, **ephíleisa*, **pephíleika*, etc., the assumption that the stem seen in **philéyō* was used throughout the verb would surely have been natural and undisputed. The phonological regularity of the developments from an assumed laryngeal suffix should provide an equally acceptable justification.

As for the source of the long vowel in the oblique forms, if it is not to be regarded as the reflex of a laryngeal, Meillet has suggested that the non-present forms are built up on the analogy of verbal adjectives containing a long vowel: *philḗ-sō*, etc. on the basis of *philē-téon*, etc.[8] The use of the verbal adjective as the base form is of course just as much a special assumption as the use of the present stem, and a priori one would take the present as a more likely source, even leaving aside phonological considerations. (In terms of the present discussion, indeed, one would be more inclined to regard verbal adjectives of the type *philētéon* as regular derivatives from the denominative stem.) But the important point is the phonological one; an explanation by analogy may be forced on us by incomplete knowledge of the phonology. Meillet has written in another context:[9]

> Le principe de la constance de lois phonétiques n'a pas été fécond seulement pour la phonétique même. . . . Tout d'abord, il a obligé les

[8] *Traité de grammaire comparée des langues classiques* (Paris, 1948), §366.

[9] *Introduction à l'étude comparative des langues indoeuropéennes* (Paris, 1937), p. 475.

> linguistes à tenir compte de l'importance de l'analogie. Sans doute on reconnaissait d'une manière générale que l'innovation analogique joue un certain rôle; mais, aussi longtemps que l'on admettait la possibilité de changements phonétiques sporadiques, il n'existait aucun moyen de déterminer ce qui lui était dû; du jour où l'on a défini quelle forme était attendue phonétiquement, il a fallu expliquer le reste, et l'on a vu qu'une grande partie de ce reste provenait d'influences analogiques.

But as our knowledge of phonology increases, and the laryngeal theory has certainly had that effect, "le reste" is progressively reduced, and we must anticipate that a number of analogical explanations will prove to be unnecessary. The non-present denominative forms in Greek, and in Latin, are a case in point; with the aid of laryngeals we are enabled to define more accurately the form that is phonetically to be expected and therefore have less need to appeal to analogical influence.

Confirmation of the conclusion that we are indeed dealing with a suffixed laryngeal, rather than *y*, is provided by a number of Greek dialect forms[10] in which the present stem of the denominative is athematic rather than thematic, so that the suffix is followed by a consonant rather than by a vowel even in the present tense. In terms of the traditional analysis—that the denominative suffix contains *y* and that it is restricted to the present stem—one would expect athematic present forms of the type **phileí-mi*, but in fact instead of the diphthong there appears here too the long vowel, in such examples as *phórḗ-mi* 'bear' and *philḗ-menai*.

The pervasive lengthening of short vowels in the denominative forms of Latin, Greek, and Sanskrit suggests that the denominative suffix contained a laryngeal rather than the *y* that has been assumed in the past. The presence of a *y*-glide in Sanskrit (and in other languages, together with the palatalization of stem-final consonants in Greek) suggests that palatal tongue position was one of the relevant features of this particular laryngeal and that this feature remained prevocalically as *y*, having fallen together with regular Indo-European *y*, previously a separate phoneme.

The second major source of evidence to be adduced rests on the analysis of Sanskrit verbs of the type *gā́yati* 'sing', which, like the denominatives, show a combination of vocalic length and prevocalic *y*. These verbs are classified by Whitney,[11] from a descriptive point of view, as being formed by the addition of a suffix *ya* to a root ending in a long vowel: *gā́-ya-ti*. The traditional Indian analysis, on the other hand, interprets the *y* as being a part of the root rather than the stem (*gāy-a-ti*), and this view has been

[10] See, for example, Carl Darling Buck, *The Greek Dialects* (Chicago, 1955), §157.
[11] *Op. cit.*, §761e.

followed by many western Sanskritists,[12] who apply the term "long diphthong" to the final phonological element of the root.[13]

The first problem then is to decide whether the *y* of these long-vowel verbs belongs historically to the root or to the suffix; the second, whether the *y* is the second element of a long diphthong or the reflex of a laryngeal. A corollary of the first problem provides a more fruitful approach: Is the suffix *a* or *ya* (*gắy-a-ti* or *gắ-ya-ti*)? From this point of view, the proportion of *ya* suffixes among verbs with roots in *ā* can be compared with the proportion of *ya* suffixes for all roots in the language to see if there is any striking discrepancy. In his *Roots*,[14] Whitney lists 133 present stems formed with the suffix *ya*, out of a total of about 883 roots; that is, about 15 percent of the roots of the language form a present stem by the addition of the suffix *ya*. The roots in *ā*, considered separately, show a very different distribution. Whitney gives 45 roots ending in *ā*, of which 17, or 38 percent, have a present stem in *ya*.[15] This remarkable increase in the proportion of apparent *ya*-presents (two and one half times the norm of fifteen per cent) strongly suggests that there is some factor other than the *ya* suffix alone that is responsible for the number of present stems showing *y* at the end of roots in *ā*. If the *y* is not easily accounted for by reference to the suffix, it would seem necessary to ascribe it to the root, and the suggestion that a number of the roots involved end in a long diphthong or some other element containing a *y* would not be unreasonable. These roots, then, would be regular thematic presents of the type of *bhắv-a-ti* 'be', characterized by a full-grade accented root and a suffix *a*: *gắy-a-ti*, not *gắ-ya-ti*.

The second problem: Is the *y* of the root to be ascribed to a long

[12] *E.g.*, MacDonell, *op. cit.*, §438a, with footnote 4.

[13] The behavior of the long diphthong is seen in the declension of the noun *rāí*, where there is regularly a *y* before endings beginning with a vowel but no *y* before endings beginning with a consonant: nom. sg. *rās*, acc. sg. *rāyam*, gen. pl. *rāyām*, inst. pl. *rābhis*, etc. Under the heading of stems ending in diphthongs Whitney (*op. cit.*, §361b) says, "The stem *rāí* . . . might be better described as *rā* with a union-consonant *y* interposed before vowel endings."

[14] *The Roots, Verb-Forms, and Primary Derivatives of the Sanskrit Language* (Leipzig, 1885).

[15] The roots are here listed, together with the seventeen present stems: *kā* 'enjoy' (*kāyamāna*), *kṣā* 'burn' (*kṣắyati*), *gā* 'sing' (*gắyati*), *gā* 'go', *glā* 'be weary' (*glắyati*), *ghrā* 'smell', *chā* 'cut up', *jā* 'be born' (*jắyate*), *jñā* 'know', *tā* 'stretch', *trā* 'rescue' (*trắyate*), *dā* 'give' (*dāyamāna*), *dā* 'divide', *dā* 'bind', *drā* 'run', *drā* 'sleep' (*drāyate*), *dhā* 'put' (*dhāyeta*), *dhā* 'suck', *dhmā* 'blow', *dhyā* 'think' (*dhyắyati*), *pā* 'drink', *pā* 'protect', *pyā* 'fill up' (*pyāyate*), *prā* 'fill', *psā* 'devour', *bhā* 'shine', *mā* 'measure', *mā* 'exchange', *mā* 'bellow', *mlā* 'relax' (*mlắyati*), *yā* 'go', *rā* 'give', *rā* 'bark' (*rắyati*), *vā* 'blow' (*vắyati*), *vā* 'weave', *śā* 'sharpen', *śyā* 'coagulate' (*śyắyati*), *śrā* 'boil' (*śrắyati*), *sā* 'gain', *sā* 'bind', *styā* 'stiffen' (*styāyate*), *sthā* 'stand', *snā* 'bathe', *hā* 'leave', *hvā* 'call'.

diphthong or to a laryngeal? Or, to phrase the question more accurately, is the Sanskrit long diphthong *āy* (from a descriptive point of view) the reflex of a laryngeal, or is it historically as well as descriptively the long counterpart of the short diphthongs **ei*, **ai*, and **oi*? If we begin with the assumption that the *y* of the long diphthong in these forms is in fact the reflex of the Indo-European first laryngeal (**H*$_1$ or **H*^{y}), we should find that in general Sanskrit roots in *ā* showing *y* in the present have cognates in other languages showing a reflex of **ē* (i.e. **eH*$_1$), whereas Sanskrit roots in *ā* that do not show *y* have cognates showing reflexes of **ā* (< **eH*$_2$) or **ō* (< **eH*$_3$). Complete conformity to this distribution cannot be expected for two obvious reasons. The Sanskrit root will not show a *y* unless it happens to form a thematic present, since, according to the normal distribution of the Sanskrit long diphthong, *y* appears prevocalically but not preconsonantically. Secondly, any root may happen to form a present stem in the suffix *ya* (as was seen above, 15 percent of the roots in the language do), so that a few roots in **H*$_2$ or **H*$_3$—like roots in *k* or *l* or *p*, for example—can be expected to show a *y* in the present stem. The hypothesis can be confirmed only if, in spite of these impediments, there is a definite bias in the distribution of *y* in favor of roots for which there is etymological evidence for **H*$_1$. Ideally, we should expect these two types of present stem to appear among the *ā* roots in roughly the same proportions as they appear in all roots of the language. This would mean that of roots in **H*$_1$, a little more than half should form presents of the type *bháv-a-ti* 'be' and should thus have the appearance of *ya* presents, and that of the roots in **H*$_2$ and **H*$_3$, about 15 percent should actually form presents in *ya* of the type *pád-ya-te* 'go'.[16]

The actual distribution is as follows: Of the 17 roots in *ā* that form presents in *ya*,[17] eleven have reasonably sure[18] cognates in other languages. Of these, eight show a reflex of **ē* (< **eH*$_1$); three, of **ā* or **ō* (< **eH*$_2$ or **eH*$_3$).[19] Of the 28 roots in *ā* that do not form presents in *ya*, 18 have reasonably sure cognates. Of these, eight show reflexes of **ē*; ten, of **ā* or **ō*.[20]

[16] Of the 883 roots listed by Whitney, 529 or 60 percent form presents of the type *bháv-a-ti* and 133 or 15 percent form presents of the type *pád-ya-te*.

[17] See footnote 15.

[18] There is of course no dearth of thorny problems in this matter of etymological relationship; by "reasonably sure" I mean here those equations that are generally accepted by the various etymological dictionaries.

[19] With cognates in **ē*: *kṣáyati*, *gáyati*, *drāyate*, *dhāyeta*, *mláyati*, *rāyati*, *vāyati*, and *styāyate*. With cognates in **ā*: *kāyamāna* and *dhyáyati*; in **ō*: *dāyamāna*.

[20] With cognates in **ā* or **ō*: *jñā*, *drā*, *pā* 'drink', *pā* 'protect', *psā*, *bhā*, *yā*, *sthā*, *snā*, and *hvā*.

	Roots in *ā* with presents showing *ya*	Roots in *ā* with presents not showing *ya*
Total	17	28
With cognates	11	18
With cognates in *ē*	8	8
With cognates in **ā* or **ō*	3	10

If we compare these figures with those to be expected from the hypothesis suggested above, we find the proportions remarkably close, considering the small number of examples available. It was anticipated that a little more than half of the roots in **ē* would form Class I presents in *a*, like *bháv-a-ti*, and hence show *y* in the present stem; exactly half, eight of sixteen, actually do. Among the roots in **ā* and **ō*, 15 percent were expected to show Class IV presents in *ya*, like *pád-ya-te*; three of thirteen, or 23 percent, actually do. But the most important item is the preponderance of **ē* roots in the first column: eight of eleven, or 73 percent. Particularly in comparison with the "normal" representation of the non-**ē* roots, this concentration clearly reveals the source of the bias that has introduced so many apparent *ya* presents into the Sanskrit roots in *ā*. This single assumption, that *y* is the reflex of a laryngeal with a palatal feature, changes a badly skewed distribution into one that is appropriate, both for the suffix *a* and for the suffix *ya*, to that found in the language as a whole.

Further, examination of a number of roots that do not form Class I presents but are nevertheless thematic—the Class VI presents with accented suffix *á* and zero grade of the root, of the type *diś-á-ti* 'point'—reveals another group of anomalous *y*'s in roots ending in *ā*. The few cases that occur can be arranged in order of transparency. The roots *sā* 'bind' and *dā* 'bind' form the presents *syati* and *dyati*. Cognate with *sā* is Lithuanian *sẽ-ti* 'bind', showing **ē*; cognate with *dā* is Greek *dé-ō*, *é-dē-sa* 'bind', showing **ē*. The roots then are **seH*$_1$ (or **seH*^{y}) and **deH*$_1$ (or **deH*^{y}); regular formation of the Class VI present—zero grade of the root and suffixed *á*—gives **sH*^{y}*-á-ti* > *syati* and **dH*^{y}*-á-ti* > *dyati*. Proceeding from these verbs, where the etymology is sure, we can assume it probable that roots with less sure antecedents but showing the same alternation between full-grade length and zero grade prevocalic *y* likewise contain reflexes of **H*^{y}. The root *śā* 'sharpen' has a clear *o*-grade cognate in the Latin noun *cōs* 'whetstone' and, as Walde-Pokorny put it (*s.u.*), "wohl **ḱē(i)* als Normalstufe." Here the Class VI present is *śyáti*, presumably from **śH*^{y}*-á-ti*, beside the Class III reduplicated present with full grade of the root, *śi-śā-ti* < **śi-śeH*^{y}*-ti*. Other examples that might be included in this group, although their etymologies are unsure, are *chā*, *chyati* 'cut up' and *dā*, *dyáti* 'divide'.

Two other verbs of interest are *dhā, dhayati* 'suck' and *mā, mayante* 'exchange', which were not listed as having Class I presents in the analysis given previously because in these present stems they do not show the long vowel in the root (one expects **dhāyati* and **māyante*) that is characteristic of the class. But leaving aside the problem of the length of the vowel, one may include them in effect in the list of Class I presents of the type *gắy-a-ti*, for both have cognates in **ē*: for *dhayate* (aorist *a-dhā-t*), Greek *thḗ-sato* 'suck'; for *mayante* (if it is to be connected with *mā-ti* 'measure'), Latin *mē-tior* 'measure'. In these verbs also the prevocalic *y* can be ascribed to **H*^{y}.

It becomes clear in this framework that the term "long diphthong" is inadequate and perhaps misleading, unless its use is sharply restricted to the synchronic description of Sanskrit itself. The term implies that there existed long counterparts of **ei*, **oi*, and **ai*. In fact, the evidence shows that there was at best a long counterpart of **ei* alone and that, more accurately, even this equation is only apparent, for the second element of the long diphthong is not a reflex of **i*, as is the second element of **ei*, but is of a different phonological origin, the reflex of which has fallen together with that of the second element of **ei*, in certain environments, at a relatively late date. That is, all that the two "diphthongs" have in common is the vowel, which in Indo-European is not much; the two prototypes are **ei* and **eH*$_1$ (or **eH*^{y}).

To summarize the evidence of the Sanskrit verbal forms, it is suggested that those present stems of the type *gāyati* that have cognates showing reflexes of **ē* be interpreted historically as Class I presents[21] of which the *y* is the regular reflex of the palatal laryngeal **H*^{y} in prevocalic position.

[21] It may be noted in passing that this assumption now provides for the existence of thematic stems for roots ending in a laryngeal. There is no a priori reason why this very common and productive suffix, the thematic vowel, should not have been added to roots ending in these particular consonants, and it is an incidental advantage of the present analysis that this anomaly is eliminated. From the point of view of diachronic morphology (on this subject see W. Diver, "On the Diachronic Role of the Morphological System," *Miscelánea homenaje a André Martinet*, vol. II, La Laguna, 1958, 41–54) an interesting problem is the survival of only one of the three laryngeal stems. The solution undoubtedly lies in the formal identification of stems of the type *gắyati* (< **géH*^{y}*-a-*) with those of the type *dấyamāna* 'give' (< **déA*^{w}*-ya-*, to use Martinet's notation). That is, the existence of a *ya*-stem present encouraged the retention of stems with *y* from the first laryngeal, for neither the phonological merger nor the resultant formal identity of the historically different stems would cause any morphological confusion, since the two present stems had differed only on the plane of expression. On the other hand there was no suffix *va* that was regularly used to form present stems and that could serve as a model for the retention of thematic stems in *va* from roots ending in Martinet's **A*^{w}. The only trace of such a formation appears to be in the "infinitives in *vane*" (Whitney,

That is, in these stems the *y* belongs historically to the root, not to the suffix, and the regular correspondence of this *y* to $*H_1$ makes unnecessary the special assumption of a long diphthong to account for the *y*.[22]

Analysis of the denominative formations of Latin, Greek, and Sanskrit and of the Sanskrit verbs with roots in *ā* has shown that these forms demonstrate a recurring connection between palatal quality and vocalic length. Previously these features had either gone unaccounted for or had been explained in various ways, not always satisfactorily, as the result of special phonological or analogical developments in the individual languages. The hypothesis that the *e*-coloring[23] laryngeal of Indo-European had as one of its relevant features a tongue position similar to that of *y* makes it possible to replace all these separate and sometimes difficult explanations by just one, which relies only on regular phonological development.[24]

The natural comparison of Martinet's labiovelar laryngeal $*A^w$ with $*H^y$ leads to the idea that $*A^w$ and $*H^y$ were somehow the counterparts of Indo-European $*w$ and $*y$, respectively, although there is as yet no evidence to suggest what relevant feature or features served to differentiate

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Grammar §974), and they form a very limited, non-productive, group. We are not yet sufficiently well-informed about the phonetic characteristics of the second laryngeal even to speculate on what might have happened to thematic stems formed on it.

[22] So also in the case of the noun *rāi*; *cf.* footnote 13 above. A root $*reH^y$ may be assumed.

[23] There has been some uncertainty as to whether the first laryngeal was specifically *e*-coloring or whether it was simply neutral, since wherever the *e*-grade of the ablaut system appeared there would be an *e* whether the laryngeal was present or not. But if we can now claim for other reasons that the first laryngeal had a palatal quality, it can be said that this quality confirmed the *e*-ness of the *e*-grade, just as the *o*-coloring laryngeal overrode the vowel of the *e*-grade (Greek *didōmi* 'give' < $*di\text{-}deA^w\text{-}mi$).

[24] There is some difference in detail between Sanskrit and the other two languages; in Sanskrit the preceding vowel is lengthened even in prevocalic position (*sakhīyáti* with long *ī*), as opposed to Greek *philéō* with short *e*. It is not immediately clear whether this is a regular phonological difference between Sanskrit and the other two or whether in Sanskrit the lengthening in this environment is morphologically determined. It is possible, for example, that *dhayati* 'suck' is an archaic form preserving the short vowel seen in Greek and Latin, while the "regular" long vowel of *gā́yati* is a secondary lengthening under the influence of athematic forms. In the Sanskrit denominative, on the other hand, the long vowel of *sakhīyáti* might well be a residue of the athematic stage surviving in isolated Greek forms and more regularly in Latin (*metūtum* < $*metuH\text{-}tom$), the long vowel being retained even after the denominatives had been shifted to the thematic conjugation by the addition of *á* to the athematic stem. Yet forms such as *octā-uus* adduced by Martinet (*op. cit.*) suggest, together with his supporting analysis, that appearance of both the long vowel and the glide in prevocalic position may be a regular phonological possibility.

the members of each pair. There is even a striking parallel in the influence of these sounds on preceding vowels: just as $*H^y$ and $*A^w$ in preconsonantal position lengthened and colored a preceding vowel to $*\bar{e}$ and $*\bar{o}$ respectively, so in Sanskrit in the same environment *ai* becomes $\bar{e}$ and *au* becomes $\bar{o}$. It is to be hoped that further research may yet reveal the phonetic characteristics both of the feature that $*A^w$ and $*H^y$ had in common as opposed to $*w$ and $*y$ and of the second, *a*-coloring laryngeal, which, it now begins to appear, may have belonged to a part of the pattern very different from that occupied by the labio-velar and palatal laryngeals.

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The Accentuation of the Verb in Indo-European and in Hebrew

Compared with other parts of speech the Indo-European verb shows certain peculiarities of accentuation, at least in the historical languages which continue the free and mobile stress[1] of Indo-European.

In Vedic Sanskrit the personal (or finite) verbal forms may be stressed or enclitic, the distribution of the two variants or allomorphs being conditioned by the position of the verb in the sentence or by the kind of sentence. In the principal clause the verb maintains its individual stress when opening the sentence, i.e. in initial position. In all other cases it is enclitic. In subordinate clauses it is always stressed. The latter fact is to be accounted for by the originally initial position of the verb in subordinate clauses not introduced by conjunctional elements; cf. English conditional sentences like *should he happen to call, he will be welcome*. Quite evidently we are dealing here with the superposition of syntactical function on primarily mechanical (phonemic) distribution. We must assume a shift of the juncture separating the subordinating elements (the relative pronoun *ya-* and the cognate conjunctions) from the following personal verb. Cf. English *I know | that he is my friend* from an older *I know that | he is my friend*. Although this shift has partly disengaged the accentuation of the verb from purely mechanical factors, the old rule can still be perceived in the principal clause.

Another important feature of the personal verb, apparently independent of its enclitic nature, was its fusion with certain preceding adverbs (thus becoming preverbs). The resulting compound was stressed on the preverb in a principal clause, on the verb in a subordinate clause.

It seems that the latter fact may explain the peculiar treatment of the finite verb, whether compound or simple. According to an opinion

[1] The term 'stress', alternating with 'accent', is used here in the larger sense, not as opposed to 'pitch', but simply denoting the prosodeme (suprasegmental phoneme) charged with "culminative" function.

expressed in my *L'Accentuation dans les langues indo-européennes* (1st ed., p. 110, 2d ed., p. 97) the fusion of the finite verb with an immediately preceding adverb (preverb) is to be considered as the *primary fact*, its enclitic character being a corollary only. If all verbal compounds, i.e. any preverb plus personal verb, bear the stress on the accessory member (=preverb), not on the constituent member (=the verb itself), the verb when separated from its preverb will be unstressed or enclitic, unless it is initial. Hence a simple verb will also be enclitic, except after a stop. This reasoning represents an application of a more general rule: Structures reduced to their constituent member are founded upon full structures (=consisting of full member plus accessory member[s]). Therefore analogical levelings follow as a rule the direction *full structure→reduced structure* (cf. *Acta Linguistica* V, 1949, p. 25).

The fusion of preverb and verb entails the basically *unaccented* character of the personal verb, its accentuation under *special* conditions (initial position, subordinate clause) being, from the *descriptive* point of view, a *secondary* fact. This relation between the primarily unstressed and secondary stressed forms accounts for a whole series of accentual anomalies hitherto unexplained in Vedic grammar (*L'Accentuation*$_1$ pp. 112–119, 2d ed., pp. 99–104).

Owing to the limitation of its accent, Greek only indirectly manifests a similar situation. Words of more than one syllable could become enclitic only if presenting the syllabic structure ⏑ ⏒. Therefore the main bulk of originally enclitic verbal forms received a secondary stress, this accent being the result of purely phonetic development, not of any morphological leveling. Originally the distribution of stressed and unstressed forms, though not an Indo-European inheritance, must have been the same as in Sanskrit. The relation between finite verb and preverb is in historic Greek rather similar to that in Sanskrit. In both cases a still existing, though limited, freedom of the preverb is in reality only an apparent one. The selection of a certain number of original adverbs, shifting to preverbs, and their semantic fusion with the finite verb, are already accomplished. There is only a difference of *degree* between the apparent independence of the preverb in the Rigveda or in Homer, and its treatment e.g. in Lithuanian, where it may be separated from the verb (say, by an intercalated negation) or in modern German, with its alternation *er bringt zurück* : *dass er zurückbringt*.

In Balto-Slavic the fusion of preverb plus verb is prehistoric. Prosodic considerations (stress, intonation) suggest the *proclitic* nature of the Balto-Slavic preverbs. This means that the accentual merging of the two originally independent words has resulted in compounds stressed on the verbal form

itself, not on the preverb. Compare the Germanic (Gothic) verbs with the preverbs *and-*, *bi-*, *fra-*, *ga-*, *tuz-*, *uz-*.

We are reminded of the above Indo-European facts when we undertake an explanation of certain accentual traits of the Hebrew verb. There are of course some fundamental differences between the two linguistic families in question which may be summarized as follows:

(1) In Hebrew (and in Semitic in general) there are no verbal compounds comparable to the Indo-European type **pro-bhereti*.

On the other hand, in Indo-European we do not find compounds consisting of a transitive verb plus a pronominal object, which are a rule in Semitic (Arabic *qatala-hu* etc.). Enclitic pronouns do not in Indo-European coalesce with the finite verb except (in the later stages) with the pronoun *-se* (Scandinavian, Baltic, Slavonic) destined to form reflexive, intransitive or passive forms.

(2) In Vedic, the crucial position of the verb, where it shows its individual stress, is the beginning of the sentence. In Hebrew, it is the final position (*in pausa*).

The parallelism between the two linguistic types lies in the existence, side by side, of compound and of simple verbal forms, and of the pressure exerted by the former on the latter. Let us develop this point in more detail.

In Hebrew a direct pronominal object is attached immediately to the verbal form. The accentual merging of these two elements results in a compound stressed on the last syllable of the verb (light suffixes) or on the suffix (heavy suffixes). Cf. the table (p. 127) (G. Beer, *Hebräische Grammatik* II, 1916, pp. 127f.):

Just as in the archaic forms of the *status constructus*, these verbal compounds in juncture form may show vowels lost long ago in the simple forms, as *qṭål-å* (3. masc.), *qṭalt-ī* (2. fem.), or inorganic ones (*qṭål-ę-*).

The shift of the accent due to the enclitic pronoun is clearly visible in the following cases (the compounds are confronted with the simples appearing *in pausa*):

	I (compounds)	IIa (simples *in pausa*)
sing. 3.m.	*qṭål*	*qåṭål*
3.f.	*qṭålaþ* (*qṭålåþ*)	*qåṭålå*
2.m.	*qṭaltå*	*qåṭåltå*
2.f.	*qṭaltī*	*qåṭålt*
1.	*qṭaltī*	*qåṭåltī*
plur. 3.	*qṭålū*	*qåṭålū*
1.	*qṭalnū*	*qåṭålnū*

	him	her	you (sing. masc.)	you (sing. (fem.)	me	them (masc.)	them (fem.)	you (plur. masc.)	you (plur. fem.)	us
he killed	*qtâlâ'-hū* *qtâlō* (contracted)	*qtâlâ-h*	*qtâ'l-châ*	*qtâl-ę-ch*	*qtâlâ-ni*	*qtâlâm* (contracted)	*qtâlân* (contracted)	—	—	*qtâlâ'-nū*
she killed	*qtâlâp-hū* *qtâlâtū*	*qtâlâtnâh*	*qtâlâp-châ*	*qtâlâ'pęch*	*qtâlap-ni*	*qtâlâpâm*	—	—	—	*qtâlâp-nū*
you killed (sing. masc.)	*qtalâ'-hū* *qtalţō* (contracted)	*qtaltâ-h*	—	—	*qtaltâ-ni*	*qtaltâm* (contracted)	—	—	—	*qtaltâ'-nū*
you killed (sing. fem.)	*qtaltl-hū*	*qtaltţ-hâ*	—	—	*qtaltl-ni*	*qtaltîm* (contracted)	—	—	—	*qtaltţ-nū*
I killed	*qtaltţ-hū* *qtaltiţū*	*qtaltţ-hâ*	*qtaltţ-châ*	*qtalti-ch*	—	*qtaltîm* (contracted)	—	*qtalti-chęm*	—	—
they killed	*qtâlû-hū*	*qtâlû-hâ*	*qtâlû-châ*	*qtâlû-ch*	*qtâlû-ni*	*qtâlûm* (contracted)	*qtâlûn* (contracted)	—	—	*qtâlû-nū*
you killed (plur.)	*qtaltâ-hū*	—	—	—	*qtaltâ-ni*	—	—	—	—	*qtaltâ-nū*
we killed	*qtalnâ-hū*	*qtalnâ-hâ*	*qtalnâ-châ*	*qtalnû-ch*	—	*qtalnâm* (contracted)	—	*qtalnû-chęm*	—	—

Our intention is to show that the accentuation of the simple forms (IIa) is based on that of the compound forms (I); and that inside the sentence the simple verb behaves, at least in a certain measure, like the transitive verb having a heavy pronominal suffix attached to it. If such be the case, then (allowing for the differences stated above) the parallelism with the accentuation of the Vedic verb will be striking.

Already the treatment of the vowel *a*, which becomes *å* in all forms of IIa, is a sure sign of the proportional pressure of I on IIa. In I the distribution of unstressed *a* and *å* is strictly phonemic (mechanical): *a* in closed syllables, *å* in open ones. From the etymological point of view *qåṭalt(å)*, *qåṭaltī*, *qåṭalnū* are the forms expected. The reason of the generalization of *å in pausa* is the fact that the phonemic distribution of *unstressed a* and *å* in I allows the uniform treatment of *stressed a* in IIa. Since in *qåṭålå*, *qåṭålū* only *å* is possible, it is also introduced into the other forms of paradigm IIa, both *a* and *å* being admissible in closed *stressed* syllables.

Transitive verbs without pronominal suffixes and intransitive verbs follow the model of the compound forms, i.e. when not *in pausa* they behave as if they were followed by a heavy pronominal suffix and are therefore unstressed. The correct structural formula is this: before zero suffix (=absence of suffix) they behave in the same way as before a heavy suffix. Paradigm IIb represents the original accentuation of the simple verb followed by another member of the sentence:

		IIb
sing.	3.m.	**qṭal*
	3.f.	*qåṭlå*
	2.m.	**qṭaltå*
	2.f.	**qṭalt(ī)*
	1.	**qṭaltī*
plur.	3.	*qåṭlu*
	1.	**qṭalnū*

These forms expected *a priori* fall under the influence of the final (=*in pausa*) forms IIa. This means the introduction of *å* in place of zero. Hence the historical forms of the simple verb when in a context (III displacing IIb):

		III
sing.	3.m.	*qåṭal*
	3.f.	*qåṭlå*
	2.m.	*qåṭaltå*
	2.f.	*qåṭalt*
	1.	*qåṭaltī*
plur.	3.	*qåṭlū*
	1.	*qåṭalnū*

The mechanism of the transformation (IIb > III) seems clear. On the model of *qåṭålå* : *qåṭlå* and *qåṭålū* : *qåṭlū* (stressed : unstressed) we obtain *qåṭål* : *qåṭal* (for **qṭal*), *qåṭålt(å)* : *qåṭalt(å)* (for **qṭalt(å)*), *qåṭåltī* : *qåṭaltī* (for **qṭaltī*), *qåṭålnū* : *qåṭalnū* (for **qṭalnū*).

In the imperfective the relation of the forms *in pausa tiqṭọlī* (sing. 2.f.), *i̯iqṭọlū* (plur. 3.m.), *tiqṭọlū* (plur. 2.m.) to the context forms *tiqṭlī*, *i̯iqṭlū*, *tiqṭlū* corresponds exactly to *qåṭålå* : *qåṭlå*, *qåṭålū* : *qåṭlū*. Except in the perfective, the vowel of the first syllable, protected by the following consonant cluster (*i̯iqṭọl*, *tiqṭọl*, etc.), is maintained under all circumstances. The phonetic variations of the verbal root are consequently less conspicuous than in the perfective.

The above data suggest as their simplest explanation an original distribution of stressed and unstressed forms of simple verbs, viz. stressed forms *in pausa*, unstressed forms before another member of the sentence. This distribution must have been ultimately dominated by the compound verb, accented on the heavy suffix or on the last syllable of the verbal form (before light suffixes).

An interesting detail confirms the original lack of stress of the forms III. The difference of treatment of the final vowel of **qaṭalta* (sing. 2.m.) and **qaṭalti* (sing. 2.f.) becoming *qåṭaltå* and *qåṭalt*, respectively, points to an *internal* position of these forms, i.e. to their *proclitic* nature. The difference of treatment is precisely the same as in the plural and in the *status constructus*. E.g. *šọfẹṭ* 'judge' : plur. *šọfṭīm*, *'illẹm* 'dumb' : plur. *'illmīm*, *'ọi̯ẹb* 'foe' : *'ọi̯bī* 'my foe', but *'ọ̄ṣår* 'treasure' : plur. *'ọ̄ṣårọ̄p*, *mišpåṭ* 'droit' : plur. *mišpåṭīm*, etc.

Owing to a stylistic (poetic) change of word-order and juncture, a verbal form whose phonemic structure proves its original lack of stress (cf. paradigm III) may happen to occur before a new pause (a caesura) and therefore receive a secondary accent. Its place will of course be dictated by the independent forms appearing *in pausa*:

in pausa		before new pause (caesura)
sing. 2.m.	*qåṭåltå*	*qåṭåltå*
1.	*qåṭåltī*	*qåṭåltī*
plur. 1.	*qåṭålnū*	*qåṭålnū*

Let us mention a closely related phenomenon of Vedic versification: a personal verb is stressed not only at the beginning of a principal clause but also at the beginning of the verse (*pada*), i.e. after a *metrical* pause, even if the latter does not coincide with a syntactical pause.

It is not possible to avoid a natural question which may, at the same time, prove an objection to the above theory. What about the nominal

accent? Nouns also combine with (possessive) suffixes and present, although in a restricted measure, special forms *in pausa*. Are we to assume that the noun also was proclitic when followed by another word of the sentence?

There is a fundamental difference between these two parts of speech. To put it in a nutshell, the ultimate reason for the difference in treatment of the verb and noun is this: whereas the direct object is postulated and presupposed by the transitive verb,[2] there is no distinction between nouns presupposing a genitive and nouns dispensing with it. The unaccented form of the noun, as it appears in the *status constructus* and before possessive suffixes, does not displace the normal stressed form of the noun, because the noun, unlike the transitive verb, is self-sufficient. Therefore in the sentence there is a distinction between stressed noun-forms and unstressed noun-forms, the latter representing the *status constructus*, i.e. noun + "genitive". And the construction noun + possessive pronoun is subordinate to the *status constructus*.

To sum up, the data at our disposal seem to point to the *proclitic* nature of the Hebrew finite verb (the infinitive *qṭṓl* included). The preservation of the old stress is to be assumed only before a stop or caesura. Both the simple forms *in pausa* (IIa) and the simple forms in context (III) must in the last resort be explained by the accentuation of the verbal compounds (transitive verbs + pronominal suffix). Interferences between IIa and III represent a later development.

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[2] And a preverb denoting direction is postulated and presupposed by the Indo-European verbs of movement.

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The Decline of German Dialects

1.1. Dialects and Standard

It has been characteristic of the nations of Western Europe that their languages show a cleavage between geographically circumscribed dialects and standard forms of speech which enjoy higher social prestige and are current over the whole territory. In the tension and interaction between these two basic types, the standard forms have been gaining ground gradually over the dialects. As long as populations were generally stationary, the development was slow. It became accelerated in recent times by the progress of modern transportation and of means of mass communication like newspapers, radio, etc., all of which favored the standard language. But in German-speaking lands the position of the dialects was until recently still relatively strong.

1.2. Postwar Migrations

A radical change in the tempo of this development can be observed in postwar Germany. Migrations on an unprecedented scale took place and broke down the isolation of dialect areas. One has to go back to the great Germanic migrations at the dawn of history to find similar population movements. The geographical range of the postwar migrations was smaller than that of the demographic turbulence in Germanic antiquity, but by no means small, since German population groups from widely scattered regions of eastern Europe were dislocated, some by administrative resettlement during the war, many more by flight and forcible expulsion after the war. The numbers involved were undoubtedly very much larger. More than ten million expellees and refugees from the east have been crammed into the narrow space of West Germany, which is less than half of prewar Germany in area.[1] Among the more than 50 million inhabitants of West Germany, more than 20 percent are recent arrivals from the east; every

[1] West Germany, with 50 million inhabitants, has the same area as the state of Oregon, with 1½ million inhabitants (M). Initials like (M) refer to informants listed at the end of this article. I must take the responsibility for reproducing their oral communications correctly. I am also indebted to many other informants, experts in various fields, who are not personally cited in this paper.

fifth West German is an expellee or refugee. Among the 17 million inhabitants of the Russian Zone, 3.8 million are expellees,[2] an even higher percentage.

The literature on recent developments in the German language has until now hardly faced the consequences of these large-scale population movements. It has been felt that it is too early to draw conclusions from them, that their consequences can scarcely be gauged at this time, and that it would be premature to do more than "surmise" (*mutmassen*) their effect.[3] The following is an attempt to take a step toward an examination of this effect in broad outlines, on the basis of observations made and oral information gathered during a study trip through German-speaking lands in 1955. I found the effect to be very strong, but different from what I had expected.

1.3. East German Dialects

Almost one-third[4] of the population units (*Stämme*), those from the various eastern regions, lost their homelands and were deliberately scattered among the West Germans, not as transplanted communities, but as isolated individuals or families.[5] This process shattered the traditional structure of their lives and destroyed the social frame for the survival of their dialects.

To be sure, after they were established in West Germany, a reaction set in. The expellees strove to renew the old ties. Homeland groups reassembled in flourishing social clubs. But this type of nostalgic association could not reconstitute the tightly knit community of living, which is lost lastingly. Here and there a vigorous spirit of enterprise gathered immigrants from the

[2] Theodor Oberländer, *Die Überwindung der deutschen Not*, second ed., 1954, p. 9. None of these statistical figures are entirely reliable because of the chaotic conditions in Germany at the end of the war; but they represent the best estimates of responsible West German sources.

[3] Walter Henzen, *Schriftsprache und Mundarten*, Berne: Francke, second ed. (rev.) 1954, pp. 7, 209. References to this book, which digests the literature thoroughly, were added to the paper after the completion of the manuscript to provide some links with previous research. The purpose of the paper is to report on personal observations and informations, not to re-examine classifying principles.

[4] Alfred Karasek-Langer, "Volkskundliche Erkenntnisse aus der Vertreibung und Eingliederung der Ostdeutschen," *Jahrbuch für Volkskunde der Heimatvertriebenen*, Salzburg, vol. 1 (1955), p. 14.

[5] *Ibid.*, p. 11. Statistics about the deliberate scattering are given by Hugo Moser, "Umsiedlung und Sprachwandel" (*Bildungsfragen unserer Zeit, Festschrift Theodor Bäuerle*, Stuttgart, second ed., 1956, pp. 122–141), a very important study which agrees in many of its conclusions with my own observations and gives valuable supplementary data on several topics.

east in newly founded industrial establishments, even with their own living quarters in newly built settlements. But such enterprises did not reassemble workers from a single eastern area, nor did they exclude native West German workers, so that the new communities did not recreate the social communities of the past. In families and in the social homeland clubs, the home dialect was frequently cultivated, even by people from the cities who had not spoken dialect before the expulsion,[6] in an understandable desire to preserve the lost values, which were instinctively felt to be linked with the characteristic speech form of the homeland. But all public addresses and transactions in the clubs were carried on in standard German, as they had been at home, and the informal conversation around the tables, if in dialect, mixed a number of homeland subdialects. The dialects of the east are lost in their old form and will most likely disappear as the older generations die off. Even if a return to the old homeland, for which many of the expellees long and continue to hope, should become possible one day, the eastern dialects would no longer be the same.[7]

1.4. West German Dialects

The West German dialects also did not remain the same. The migration meant not only the dissolution of old social structures in the east but the formation of new ones in the west.[8] The traditional separation of German population groups in geographical compartments of homogeneous dialect speech ceased to exist. No community of the west remained unaffected by the influx of new eastern residents. This changed the social structure[9] and influenced the local dialect, which was no longer left to

[6] One of my informants (L) reported this situation for his own family.

[7] K 1 has already heard arguments in families from the east about their former speech forms: "How did we use to say that?"

[8] Eugen Lemberg and Lothar Krecker, *Die Entstehung eines neuen Volkes aus Binnendeutschen und Ostvertriebenen*, Marburg, 1950.

[9] Sociologists are well aware of the radical changes in the social structure of postwar Germany. The leading sociologist Helmut Schelsky lists, among the tasks of present-day sociological research, "eine sozialwissenschaftliche Tatbestandsaufnahme auf allen Gebieten unserer bis in die intimsten Strukturen erschütterten und veränderten sozialen Wirklichkeit." He mentions the expellees as the first element in the structural change ("Die Soziologie in der Praxis," in the symposium, *Deutscher Geist zwischen gestern und morgen*, Stuttgart, 1954, pp. 262–280). Without emphasis on the newcomers, the very active sociologist K. Valentin Müller, himself an expellee from Prague, describes the erosion of the old middle class and the formation of a new middle class from technologically skilled workers. He says the social goal of laborers' sons is the middle layer of industrial employees, who have taken over the forms of the old middle class in modified shape (*Die Angestellten in der hochindustrialisierten Gesellschaft*, Cologne, 1957). The social rise is undoubtedly accompanied by a change of speech type.

itself without outside interference (K 2).[10] Even in cases where Germans from the east moved back into areas from which their ancestors had emigrated centuries before, their customs and dialects were no longer identical. The long separation had led to divergent developments, and the individuals "returning" from the east did not fit easily in the ancestral social structure and speech form.

The thorough mixture of different populations in West Germany—no city or village being without its quota of expellees and refugees—and the clash of the newcomers' dialects with the local dialects did not, however, lead to a mixture of dialects as a rule. In my travels all over West Germany, Austria, and Switzerland, I had planned to collect evidence of the interaction of dialects. I found very little. If old settlers and newcomers had spoken only their dialects, a clash and eventual compromise might have resulted. In this struggle of dialects, to be sure, the settled dialects would have had an advantage. They had vested rights in their habitats and were not disturbed by the shattering experience of being uprooted from their soil. Also, the newcomers, although numerous in every community, were as a rule a minority, not homogeneous among themselves and in a socially depressed position, with little prestige, at least in the beginning. It would have been natural for the locally established dialects to resist interference from the outlandish intruding dialects, and for the speakers of eastern dialects to adapt themselves to a considerable extent to local speech in order to gain a foothold in the new home. Still, the influence could have been expected to work to some degree in both directions.

2.1. Strengthening of Standard

This, however, was not the case to any considerable extent. Germans, both those in the west and those from the east, had previously spoken not only their dialects but a form of standard German as well, which they had learned in school and used regularly for higher functions in life and in speaking with strangers not native to the soil in a narrower sense. In fact, in larger communities, in the west and particularly in the east,[11] a colloquial form of the standard, more or less tinged with local dialect features, had

[10] Around 1810, only the minister and the teacher spoke standard German in the village; all others spoke dialect (K 2). The situation changed slowly in favor of the standard since then, rapidly since the war.

[11] Cf. Erhard Riemann, "Die Ostvertriebenen und ihre Sprache," in: Richard Mehlem, *Niederdeutsch*, Hannover, 1956, p. 30: "Diese Auseinandersetzung war vor 1945 in Ostdeutschland viel weiter fortgeschritten als im deutschen Westen. In Ost- und Westpreussen wurde in den sozial gehobenen Schichten überhaupt nicht mehr Platt gesprochen . . . auch auf dem Lande."

been used even in ordinary pursuits. When the old and the new elements of the population met, the natural way out of the difficulties which stood in the way of perfect communication was to resort to standard German, which was familiar enough to both, and was for both the natural form of speech to use with strangers. The result was a strengthening of colloquial standard German and a dwindling of the dialects.

The resistance of the dialects varies with the conservative or progressive attitudes of their speakers. In a rough division it may be said that the southern dialects have stronger vitality, whereas the northern dialects recede at an astonishing rate. A contributing reason is perhaps that Low German is too different from High German to be understood by those newcomers whose speech was not Low German. But the most striking impression in traveling through Germany today is that one hears everywhere, in the south as well as in the north, from adults and children, prevailingly standard German in the street, where one expects, on the basis of earlier experience, to hear dialect.

2.2. Provincial Colloquial Standards

The gain of the standard does not so much affect the speech of educated people, who have long favored it on all levels of their lives, as that of the less educated ones, who used to cling to dialects or strongly dialect-tinged speech. Their standard speech is of course not the same as the intellectual standard German of the educated, which is close to the literary language, but a colloquial standard which retains a reduced dialectal tinge, now valid over wider areas than it used to be. The dialects are by no means extinct; but if the movement continues, severely restricted local varieties of dialects, and later the dialects in a larger sense, will give way to provincial colloquial standards of larger geographical scope.[12] This development

[12] Mitzka's term "traffic dialects" (*Verkehrsmundarten;* cf. Henzen 199) would cover leveled dialects valid over wider areas rather than local subdialects. As Henzen (197f.) states, there is no sharp boundary line between dialect and colloquial; the transition is gradual. A purely descriptive analysis of any speech type would not need to worry about assigning phenomena to either dialect or standard colloquial. It could simply state facts without tracing their provenience. In a larger conspectus which follows the lines of the linguistic development, the difficulty must be faced. Henzen (201f.) makes, for cities, a social distinction between two types of colloquial: a preponderant "lower" type, phonetically under the influence of dialect, which prevails in the south, and an as yet less common "upper" type, closer to the educated standard (*Hochsprache*), which predominates farther north. He states specifically (202) that the lower type is beginning to develop in and around Zürich, although in general Switzerland lacks the colloquial middle layer (199).

would probably have taken place without the influx of refugees, but their presence everywhere increased the tempo of the process greatly.

2.3. City and Country

Big cities, drawing their population from various dialect areas of the surrounding country, started the trend long ago.[13] A more recent development is the reversal of the process: standardized city speech radiates into the surrounding countryside, even around smaller cities.[14] Around Villach (Carinthia), early in the present century, the farmers' dialect began at the city gates; now city speech is used there (Kr). Around Klagenfurt (Carinthia) the country speech used to be Slovenian; now no native Slovenian speakers are found there any longer: the countryside speaks German (Kr). This movement has become vastly more important since the war. Because of the shortage of living quarters in the city, many factory workers, especially refugees, live in the villages around the city and commute daily or weekly between the city where they work and the village where their families live. For them the shop language becomes more important than the local speech, and the shop language is generally based more and more on standard German (K 1). Commuter speech is now an important ingredient of the language. There is, to be sure, a tendency for the workers to fall back into dialect as they approach home, as if subconsciously they desired to distinguish between the workaday speech and

[13] A good example outside of German is the city of London, which drew many of its speech features from neighboring West Saxon and Midland dialects, from medieval times. In German speaking lands, the speech of big cities has long been a compromise from many sources, not so much a mixture of speech forms as a leveling off of the most striking features of the contributing dialects and a strengthening of the standard language. Examples are Berlin, Hamburg, the Ruhr district. (For the latter, see the publications of the sociologist Wilhelm Brepohl, especially the latest: *Industrievolk im Übergang; Wandel von der agraren zur industriellen Daseinsform, dargestellt am Ruhrgebiet*, Tübingen, 1957.) For Stuttgart, which retains so many dialect features that it appears to the north German as a citadel of Swabian, an informant (S) told me that his co-workers in the city recognize his Swabian as genuine, but an old woman in a village only 5 km away said to him contemptuously, "You are not even from Stuttgart": big city speech is not felt as genuine by real dialect speakers. In Vienna (Kr), the dialect mixture led to the abandonment of the striking features of all contributing dialects, and the numerically strong influx of non-German speakers (especially Czechs, 1848–1910) who learned to speak German in Vienna, promoted further leveling. See also Eberhard Kranzmayer, "Lautwandlungen und Lautverschiebungen im gegenwärtigen Wienerischen," *Zeitschrift für Mundartforschung* XXI (1952/53), 197–239. For the debatable terms "Stadtmundart" and "Halbmundart" to designate city colloquials more or less affected by dialect, see Henzen 203ff., with literature (also his index).

[14] Cf. Henzen 208ff.

the relaxed home speech. It was reported several times that conversations on commuter trains would begin in the colloquial standard of the shop, but revert progressively to dialect as the travelers approached their homes. For the newcomers from the east the relaxed speech is likely to be the old home dialect of their parents rather than the local dialect of the new home, although the younger ones among them may have learned the latter in their schooldays—a temporary reversal in the process of assimilation. The gainer is the standard language, which penetrates the speech of the shop with its mixed group of workers as well as that of the village with its mixed population. The tendency to desert the country and to move to the city (*Landflucht*) is counteracted by a trend toward country residence and even country industry (K 1). The new settlements which have been built at the edge of a great many villages, primarily but not exclusively for refugees and expellees, have their center of gravity in the nearby city (K 2). Rivers and great highways were important arteries for the spreading of linguistic forms in olden times (Kr). Now the commuters' railroads have the same function (K 2), and with increasing motor traffic the roads begin to be important again.

2.4. Variations Within the Standard

Taking a closer look at the speech form which is gaining as the result of all these changes, we must keep in mind that the spoken form of standard German has not reached the same degree of uniformity as the written language, either in pronunciation or in vocabulary. Kretschmer[15] showed the great diversity of terms, particularly for matters of everyday life which are not effectively controlled by the written language, as of 1909–1915. At that time an impressive number of terms were geographically restricted in their currency, although the interpenetration had begun. In a recent collection of colloquial expressions by Küpper (1955)[16] the unity of colloquial speech could be assumed and provincial origins of expressions could be subordinated as historical background. Küpper (pp. 12ff.) emphasizes the leveling effect of large-scale interior migrations, especially since World War II. In a prospectus enclosed in the book, inviting volunteer contributions of readers, the expellees are mentioned as a separate category of speakers; but they do not yet figure in the book itself.

[15] Paul Kretschmer, *Wortgeographie der hochdeutschen Umgangssprache*, Göttingen, 1918.

[16] Heinz Küpper, *Wörterbuch der deutschen Umgangssprache*, Hamburg, 1955. Although the aim of the book is not the same, it is strange that Küpper does not even mention Kretschmer in the list of his precursors.

2.5. Meeting of Eastern and Western Standards

When the eastern Germans came to settle among their western compatriots and both groups resorted to the colloquial standard language as a common denominator, their forms were not exactly alike in all details. There were certain differences in vocabulary and idioms, and dialect substrata had their effect on the pronunciation. Local coloring of the colloquial standard had not been an obstacle to communication as long as the speakers conversed mostly with speakers from the same area. Now however speakers from entirely different areas came to live together, and the differences became noticeable. They hardly affected communication as such; in most cases the speakers understood and were understood. Rather, the effect was psychological and sociological: the feeling that the others spoke somewhat differently was an obstacle to integration. The newcomers did not feel fully at home in the new environment, and the old settlers were not immediately ready to accept, as their own kind, people who did not speak just like themselves. The homogeneity for which any group strives had to be achieved with a struggle.

2.6. Process of Compromise

The first things to go were differences in the newcomers' speech which gave the most striking impression of strangeness: definitely local expressions and conspicuous variations of pronunciation. Even in the speech of the highly educated, remnants of dialectal enunciation used to be common. Now it is often difficult or impossible to detect which region an educated speaker comes from. Among less well educated speakers, too, the differences of pronunciation become gradually less noticeable.

As to strikingly different regional words and expressions, it would sometimes happen as a consequence of the recent mingling of populations that an utterance containing one of them was not immediately understood and had to be explained. By this process the unfamiliar word became known among speakers who had not known it before. Many of the regional terms discussed by Kretschmer are now known over much wider and geographically separated areas; they are not necessarily used but at least understood. I have given examples of this dispersion in a previous article.[17] Where two synonymous terms met, one of them had to yield, unless a fully standard word was available on which the two groups could compromise. In the nature of things it was usually the word of the newcomers which gave way. The newcomer did not want to appear different.

[17] "Recent Developments in the German Language," *Journal of English and Germanic Philology* LVII (1958), §§1.1, 1.2, and 5.

The entrance into a closely knit community was difficult enough without emphasis on differences in customs and speech. So the next time he avoided an expression if experience had taught him that it gave offense. Many of the newcomers were not willing to adopt the dialect of the new home; they had their pride and did not yield so easily to the ways of others; but to substitute a standard item, when one was available, did not mean giving up one's heritage, and perhaps even added prestige, because the standard language enjoys prestige everywhere. Nor was the standard version as offensive to the old settlers as a regional equivalent not current in the area. But hearing the local term constantly would eventually lead to its adoption, passively first, later actively. This is a facet of the slow, painful integration process. On the other hand even the local people, whose entrenched position gave their form of speech much more strength, occasionally came to use a word of the newcomers, if only in quotation marks, so to speak: "as you people say," "as the refugees say."[18] The grocer, who used to be asked for potatoes by one local term, now responds readily to six and more names for the potato, with which refugees and expellees ask for them (K 1). Of course, such multiplicity of terms cannot last. The motley variety of names for tools, for instance, becomes simplified and standardized (K 1)—a loss in color, but a gain in unity.

2.7. Levels of Colloquial Standard

It is important to keep in mind that the standard speech even of the same individual, in Germany and elsewhere, is not an invariable entity. It accommodates itself to the person or group spoken to. The colloquial standard of an individual has several layers suitable for a variety of social situations.[19] The intrusion of dialects is merely a special aspect of this condition. It is parallel to the intrusion of foreign languages into the English of immigrants in the United States (K 1). In Germany as in the U.S., the speech of the old settlers, into whose society the newcomers must fit themselves, is bound to win out, with only minor enrichments from the speech of the new arrivals. An important difference between the two situations, however, is the fact that in Germany the two groups do not speak two quite irreconcilable languages, but have as a common meeting ground the colloquial standard speech, which is strengthened by their

[18] Informant L reports two such experiences. His native east Swabian landlady told him, "Tante Anna [a refugee] bäckt heute *Nudla—Buchteln*" (the Austrian name for the yeast dish, of Czech derivation, added to the local name for L's benefit). A farmer speaking with a refugee woman used the local word *Stadel* 'barn'; when she did not understand he added, "Ja, wie ihr sagt: *Scheune*."

[19] Cf. Henzen 23, 197.

intercourse and progresses on the way to unification. A colloquial standard which has its locus between the dialects of home life and the book-based speech of the educated has long been in the process of developing. It received a powerful boost by the mingling of populations in the wake of the second War. The old call for a respectable middle language[20] is beginning to be answered.

3.1. Dialect Survival

Thus, a leveling of the colloquial standard was a consequence of the mingling of populations. Of course, where the position of the local dialect was still strong, complete integration on an intimate level required that the newcomers learn the dialect of the old settlers. Anyone who speaks differently, even though he is fully understood, is not felt to be a member of the group. Social and economic reasons often demanded full conformity. The rate of adaptation to dialects varied greatly with the psychological and social attitudes of the individuals. Older people are less given to learning new tricks than younger. Women have a tendency to be more conservative than men, and the tendency asserts itself strongly when they stay at home and do not mingle much with outsiders. On the other hand, if they go out to work, they often adapt themselves more readily and more successfully to the new surroundings, particularly the younger women. Young people are most likely to conform to their present environment. They have no strong attachment to the old home, which they remember only dimly and often unfavorably, because their memory reaches back only to the terminal stages of the old life, with its insecurity, persecution, and hardships. They do not share the nostalgic longings of their elders. They live in the present and the future. Besides, the groups of young people with whom they live are much less tolerant of differences than those of older people. The schoolyard community of children particularly enforces conformity subtly or crudely. There are many reports that children were forced to mend their speech ways by lickings or by ostracism. Thus, in strong dialect areas, the children of the expellees usually learned the new speech, half willingly and half under compulsion—sometimes even one dialect after another, if they

[20] Cf. Hugo von Hofmannsthal, preface to *Wert und Ehre deutscher Sprache*, 1927: "Wir haben eine sehr hohe dichterische Sprache und sehr liebliche und ausdrucksstarke Volksdialekte, von denen die Sprache des Umgangs in allen deutschen Landschaften verschiedentlich angefärbt ist. Woran es uns mangelt, das ist die mittlere Sprache, nicht zu hoch, nicht zu niedrig. . . . Unsere Nachbarn, Nord und Süd, Ost und West, haben sie. . . . In dieser mittleren Sprache aber fasst sich allezeit das Gesicht der Nation zusammen." This observation echoes much older ones in his anthology, by Justus Möser, Herder, and Adam Müller (pp. 55, 74, 140 in the 1957 reprint, Fischer Bücherei). Adam Müller has the best formulation.

were resettled several times, as often happened. The only mitigating element in this painful process of adaptation was the fact that the autochthonous population itself shifted at the same time more and more to standard colloquial speech, thus providing a more neutral meeting ground. In cases where the dislodged had to live successively in six or ten places, they naturally gave up learning each new dialect. Their speech became more and more standard without their being conscious of it (B, K 1).

3.2. Conflicts Between Adults and Children

The differences in the attitudes of the generations led to many conflicts and psychological difficulties. In countless families, particularly those of rural origin, the parents insisted on maintaining the home dialect within the family and forbade their children to speak the dialect of the new homeland. It could happen that a child was punished by the parents for speaking the dialect of the new environment, and by the playmates in the schoolyard for not speaking it. The parents did not, however, object to the children speaking standard German,[21] and the playmates likewise felt a less strong aversion to a form of speech which for them, too, was an ideal cultivated by the school and basically recognized by them. The consequence was that many children of the newcomers became trilingual in the sense that they spoke the old dialect at home, the new dialect in the play group, and a form of standard in the classroom and elsewhere. The strongest of the three forms of speech was the standard, which radiated into both dialects. Even children willing to speak the parental dialect at home did not succeed in speaking it in pure form. Unconsciously they allowed it to be modified by the newly learned dialect or by the standard, and the parents themselves yielded to such influences without realizing it. This fact bears repetition: the dialects from the east are no longer the same, even where they are loyally preserved.

The conflict in a young speaker's mind is illustrated by the question of a seven year old boy to his father: "*Poppe, soll i schwätze, soll i sage, oder soll i spreche?*" The anecdote, reported from Swabia,[22] shows the child's consciousness of regional differences in the word for 'to speak': *schwätzen* Swabian, *sɔgen* Bavarian, specifically a Lower Austrian dialect near Budapest (B), *sprechen* standard. It also shows the unconscious adaptation to Swabian, particularly in the dropping of the *-n* in all three infinitives, a

[21] There were many cases of parents insisting that their children speak standard German rather than any dialect. (Cf. Lemberg and Krecker, p. 43; see note 8 above.) Thus standard German was reinforced in the families.

[22] Hermann Bausinger, "Beharrung und Einfügung," *Jahrbuch für Volkskunde der Heimatvertriebenen*, vol. 2 (1956), Salzburg, 1957, p. 13.

feature which is characteristic not only of the dialect but of the easy colloquial standard of the region.

3.3. Adolescents

It is not possible to draw a simple picture of the process of linguistic assimilation. Too many personal and social variables enter into it.[23] What is given here is an average drawn from a great number of detailed observations made by me and my many informants. One complication is this: as mentioned before, young people who go to work every day learn the speech of their co-workers, which has greater authority for them than the home speech. After the stage of adolescence is over and the rebellion against parental authority subsides, the young newcomers often revert to more respect for the traditions and speech of the old homeland, and show more willingness to speak the home dialect in the family circle as well as they can. This creates a cross current retarding the assimilation. As long as the old folks are living, these young people waver between the homeland speech, which may be dialectal or a variety of the standard, and the shop speech, which is a different variety of the standard, influenced by a different dialect.

4.1. Collection of Source Materials

It is symptomatic that the scientific study of old customs and old dialects is being undertaken energetically at this stage. Whenever extinction threatens a specimen, the museums become interested in it. There are numerous scholarly organizations which are eagerly at work to preserve the heritage of the dispossessed from the east.[24] The emphasis is on folklore, but several include the study of the waning dialects of the east. Their archives of tape recordings contain valuable source material, which can be exploited by dialectologists even after the dialects have ceased to exist. The feeling of urgency to preserve a record of dialects which are fast disappearing has led German scholars to assign priority to the recording of eastern speech, as long as individuals can still be found who have retained their native way of speaking relatively unimpaired. I heard several such recordings in different parts of Germany. What impressed me most was

[23] Perhaps the demonstration that linguistic changes are a complicated process is one of the values of a study showing a change in progress. For remarks about complexities in the development of the English standard language, cf. E. J. Dobson, *English Pronunciation 1500–1700*, Oxford, 1957, p. v.

[24] For names, addresses, and personnel as well as for accounts of their activities see part II of *Jahrbuch für Volkskunde der Heimatvertriebenen*, Salzburg, vol. 1 (1955) and vol. 2 (1956).

the fact that the choice of vocabulary and idioms even of good dialect speakers was to a considerable extent "tainted" by standard German, particularly in north Germany.[25] This, however, is not necessarily an effect of the postwar mingling of populations. The influence of the standard language on the dialects was under way before the recent upheavals.

One large scholarly enterprise embraces the old forms of speech in the west as well as those which have come from the east. Dr. Eberhard Zwirner's *Deutsches Spracharchiv* (founded in Brunswick, now located in Münster) is devoted to the ambitious plan of collecting tape recordings in 1200 localities of West Germany from six speakers in each: three old residents and three expellees; old, middle-aged, and young; rural and middle-class; dialect and colloquial speech. The aim is to catch the leveling of speech forms in the making as well as to preserve records of the old dialects. The project is well under way with the aid of public funds and the collaboration of local experts, Germanists and others, in the various regions; it is to be finished by 1959. Zwirner has begun to publish the records[26] and will evaluate them phonetically with his own phonometric method. Other scholars can use them for other linguistic purposes. The collection of materials from this period of transformation promises to be of lasting value. As yet little has been done by linguists to study the impact of the influx from the east on the language in the west—far less than by folklorists, who have found significant interpenetrations. Yet the opportunity for observing the linguistic effect of large-scale migrations in the making is unique, and the opportunity is passing rapidly with the progress of assimilation. Many observers in Germany and Austria feel that it is too early to study the effects, since nothing is as yet settled. I think, on the contrary, that the passing stage of turmoil ought to interest the linguist. Historical linguistics has always been handicapped by having only finished changes to study, being reduced to guesswork and arguments of plausibility for their explanation. Here for once we have a chance to observe changes as they are going on, and they prove to be much more complicated than the simplified abstractions of traditional formulations. Since little has been done by linguists during the period of transition, the recordings made during this time should prove to be very valuable in the future.

[25] Cf. Henzen 193ff. for infiltration of standard words, sounds, and forms into dialect, with literature.

[26] *Lautbibliothek der deutschen Mundarten*, Göttingen, 1957– . For an account of the plan and its precursors since 1899 see Eberhard Zwirner, "Lautdenkmal der deutschen Sprache," *Zeitschrift für Phonetik und allgemeine Sprachwissenschaft* IX (1956), 3–13.

4.2. Example: Swabian in Württemberg

A few exact studies are available, which investigate the decline of the home dialects of eastern refugees and expellees in their new western abodes, and incidentally throw light on the strengthening of the colloquial standard.

Ulrich Engel, a student of Hugo Moser, has published a good examination of the speech of expellees as it has been affected by the encounter with Swabian speech in Württemberg.[27] He feels sure that there is a slight influence from the newcomers on the speech of their present environment, but he restricts his examination to the reverse influence.

Among the expellees (roughly one fifth of the population of the land Württemberg-Baden) the Sudeten Germans (from Bohemia) predominate with about 50%, followed by Germans from Hungary, 15%, and Silesians, 10%. The former homes of the remainder extend all the way from Yugoslavia to East Prussia and Russia. The newcomers were not settled as units but scattered over the land, and their home dialects were very different from each other. The Hungarian Germans alone, for example, spoke either Franconian or Swabian or Bavarian dialects. Württemberg-Baden itself has Swabian and Franconian dialects; but in this study we learn only about the clash with several Swabian dialects of the west, and with the Württemberg colloquial language, which is a compromise between Swabian and standard German.

Engel finds, as was to be expected, certain variations depending on age, social standing, residence, occupation, and individual attitudes, whereas the effect of sex and denomination is negligible. These variations are not stated in general terms but documented by concrete examples. He examines five cases in considerable detail. Two of them concern a single person. The others are families, one of them a large double family of eight related persons. All were village dwellers and dialect speakers in their former homes in various parts of Hungary (some of which later became parts of Yugoslavia). Their dialects differed from each other. In one case it was Swabian, in three cases varieties of Bavarian, and in one case Rhenish Franconian. Some now live in cities (Reutlingen, Tübingen, Stuttgart), others in a village, and one family was still (1954) in a refugee camp. Most of them went through a period of shifting residence in other dialect areas before becoming settled. In each case, salient features of the home dialect

[27] Ulrich Engel, "Die Sprache der Heimatverwiesenen und das Schwäbische," *Württembergisches Jahrbuch für Volkskunde* 1956, Stuttgart, pp. 90–111. The same annual contains, in a survey of the literature 1954–56 by Irmgard Hampp and Hermann Bausinger, an account of recent publications about the dialects and popular speech of Württemberg (pp. 140f.).

are stated briefly but adequately, with emphasis on sounds, but also some attention to forms and syntax. Then the changes in the speech of each individual are characterized. The results differ: some speakers preserve the home dialect, usually persons over 40; some speak both dialects, keeping them separate; some speak a dialect mixture. Engel observed that school children learn the new dialect to perfection, but does not state clearly whether any of them speak only the new dialect. The strengthening of the standard colloquial language is not directly evident in these cases of rural speakers. But going beyond these cases, Engel says that those who already spoke standard German in the old home (all Baltic Germans, nearly all north Germans, many Silesians, part of the Sudeten Germans) strive to acquire the colloquial standard in the form it takes in the new home, whereas dialect speakers (almost all Balkan Germans, most Sudeten Germans) incline toward the dialect of their new home.

Generalizing on the basis of his earlier, more comprehensive research (E), Engel examines the average reaction of speakers of different regional provenience. He finds that Low German dialects, which were already receding before the standard language in the old home, survived practically nowhere in South Germany. The Franconian speakers, intellectually flexible and adaptable, gave up their home dialects easily, because, as he says, their dialects differed least from the standard language; by implication this observation does testify to a strengthening of the standard. The Swabian dialects maintained themselves best, although they differed from the Swabian of the new home, having undergone a strong influence of neighboring Franconian dialects in the east. The speakers were not conscious of the minor differences between the old and the new forms of speech and therefore did not change their speech habits quickly.[28] Bavarians, known for their conservative linguistic attitude, tended to retain some of their speech peculiarities, even the younger speakers who had otherwise switched to Swabian.[29] The result of these adaptations made by the newcomers was either bidialectism or dialect mixture, depending on circumstances and on the attitudes of the speakers.

[28] Orally Engel remarked (E) that the old settlers noticed the difference in the Swabian of the newcomers but did not object to it, being used to the presence of refugees in their midst. Antagonism toward the newcomers was due to government favors extended to them rather than to dialect differences.

[29] Oral communication (E): Sudeten Germans with Bavarian dialect adapt themselves slowly to Swabian. Both forms of speech being conservative south German dialects, the need for adaptation is not urgent. Since the differences are considerable, however, a development takes place eventually. The speakers either learn Swabian, retaining their Bavarian as a separate entity, or Swabian elements intrude slowly into the Bavarian dialects. Refugees are only marginally considered in Engel's dissertation

4.3. Example: Sudeten German Dialect

Another report, by Kurt Langer, a student of Ernst Schwarz, deals with the development of certain Sudeten German dialects transplanted from eastern Bohemia to West Germany.[30] The author, who had already made dialect studies in the old homeland, visited the expellees from his small home district now living in nearly all parts of West Germany, more than 1000 persons in about 100 localities. He, too, speaks repeatedly of the mutual influence of old and new dialects, but concentrates his attention on developments in the transplanted dialects.

The dialects of his homeland on the Oppa river, a tributary of the Oder, were better investigated than some other German dialects in the east. German settlers had been called to the area by local rulers in the 13th and 14th centuries to clear the land and bring it into cultivation. By the criterion of dialect features, they must have come in two streams, some from Hesse, others from the eastern Main river, with an admixture of Bavarians. The leveling of the various Franconian (and Bavarian) dialects had been going on for seven centuries, to the time of the expulsion, 1945–46.

The expellees coming from Austrian (Czechoslovakian) Silesia were conservative in their attitude and liked to speak dialect. Even those with higher education or in better social position were not ashamed of it. But the scattering of the population (*Zersiedlung*) all over the west robbed the dialect of its sociological support in a coherent body of speakers. The newcomers felt more at home in the south of Germany than in the north; but here they collided with solidly conservative dialects which opposed themselves powerfully to their own form of speech. Yet, the dialects resisted assimilation better in the country than in cities, where the influence of the standard language militates against the retention of any pure dialect;

(E). Its chief topic is the demonstration that the area around every city in Württemberg speaks, in addition to the local dialect, two forms of the colloquial language: one which is close to the standard, the same in all of Württemberg, and another provincial one, which leans more heavily on local dialects.

[30] Kurt Langer, *Die Entwicklung der Mundarten der Vertriebenen des Oppalandes in der westdeutschen Umwelt*, 1953 (typescript). The linguistically trained author, incapacitated by tuberculosis, undertook the study as occupational therapy. This paper, made available to me by the author, gives the general part of his studies concerning the dialects of his home district in Austrian (later Czechoslovakian) Silesia, as a specific example of the development of Sudeten German dialects in the west. Detailed studies are to follow. The Sudeten Germans in general were called to Bohemia and Moravia in the thirteenth century by local rulers and landowners to clear waste land around the edges of the then inhabited territory, toward the surrounding mountains (cf. Ernst Schwarz, *Sudetendeutscher Wortatlas*, Munich, 1954, vol. 1, p. 11).

only the old people (60–80 years), who lived in their small family circles, largely isolated from the environment, maintained their old dialect even in the cities. In the country the imported dialects maintained themselves for a time; but the fact that the newcomers did not all come from the same locality in the old eastern home district had the effect of leveling the differences between local varieties in favor of a district dialect which had not existed in this form before the expulsion.

Langer emphasizes that the dialects had been changing from generation to generation before the expulsion, in recent years especially through contact with other speakers in the labor service and in the army. The transplantation is reflected in speech as all previous vicissitudes of fate had been. The change of generations is decisive for the adaptation of the imported dialects to the new environment. Children up to the age of 14 no longer understand the most characteristic words of their parents' dialect. The score of youths up to age 20 in answering test questions is little better, although traces of the parental dialect in their speech show that they may retain more of it subconsciously. Langer questioned about 100 children and young people in the Allgäu district of Bavaria and found that the old characteristic equivalents for one noun ('potato') and two verbs ('toil' and 'get drunk') were remembered only by speakers over 20 years of age. Speakers over 20 generally retain the home dialect, but speak a colloquial form of the standard language with the old settlers and with expellees from a different region. These are average conditions, which are subject to variation in individual cases, depending on occupation, character, cross-marriages, and other specific circumstances.

As the first sample of his specific observations in various regions of West Germany, Langer deals in this paper with some effects of the transplantation to the Memmingen–Mindelheim area of the Allgäu (western Bavaria), where expellees from the Oppa land formed almost 30% of the population and lived mostly in rural surroundings. This produced a decided clash of dialects, with less influence of the standard language. The Franconian and Bavarian dialects of the old home collided with the Swabian-Bavarian dialect of the new habitat. Both parties were at first impressed by the fact that the other group spoke differently. A feeling of strangenesss prevailed, which had to be overcome slowly, not without a stage of mutual mockery, distrust, even hostility. Linguistic misunderstandings occurred, because some expressions were not even mutually understandable. The children in school, more adaptable and more exposed to coercion from their playmates, switched to the dialect of the new home. Some parents tried to forbid the use of the new dialect in their house, but could not stem the tide. Through the children and through contacts of daily life the older people

soon became acquainted with the differing speech forms of their new environment, learned to understand them, and eventually to use them.

The result among people over 20 was the elimination of dialect expressions from the old home which were strikingly different, and a slow adaptation to the vocabulary, idioms, and even sounds of the new home. However, as Langer emphasizes, these older people in the country chose to resort to the standard language as spoken around them rather than learn the new dialect. Wherever there was a choice in terms, the newcomers favored standard German. It is easy to see that the presence of standard speakers in large numbers in the villages and on the farms of a district where dialect had been the medium of normal everyday communication, was bound to lead to a considerable strengthening of the standard, which had formerly been almost a foreign language learned in school and used rarely, with outsiders. In the cities, of course, the influence of the standard had been strong before. This study is valuable because it shows how the influx of expellees carried the process of linguistic standardization into rural speech, even in the conservative south.

Langer gives a considerable number of examples of old-home expressions which were lost and replaced, and a few instances of features of pronunciation (sounds) which were modified by a compromise between dialect and standard. Space does not allow citing many of them. *Vesper* 'afternoon lunch' became *Brotzeit*; *Schwerbauch* (sic!) 'fat stomach' became *Wambe* or *Ranzen*; *ein wenig* became *ein bissla*; *Parapluie*, already receding in the old home before standard *Regenschirm* (in dialect pronunciation), was lost and largely replaced by local *Regadächla*; *Scheune* or *Scheuer* was superseded by the definitely Bavarian *Stadel*; [ropɔr] *Radeber* 'wheelbarrow' became *Schiebkarra* or standard *Schiebkarre*; *Erdäpfel* 'potatoes' was displaced by various forms of *Bodenbirnen*, including this "standard" form. Velar [l] disappeared, and the east Bavarian vocalization [i] of /l/ before consonant (as in *Wald*, *Feld*), already recessive under standard influence in the old home, was given up. Local Swabian *woischt* 'you know' was taken over by expellees in the half-standard form *waischt*. The most striking example of a newly learned word (by no means in standard meaning, to be sure) is *Hura* as a derogatory epithet of so general a meaning that it can be used good-naturedly in the Allgäu. We can imagine the shock which the expellees felt when they first heard themselves called *Huraflüchtlinge* or one of their girls *Huramensch*. The initial shock was an effective teaching device. They soon learned to use the word themselves. Coarse terms of the local dialect are learned easily by the newcomers because they are striking. The pleasant-sounding local word *Fiedle*, *Fiedla* 'anus' was adopted even by older expellees. Otherwise the latter com-

promised by favoring the colloquial standard. This contributed to a weakening of both the old and the new dialects. At the meetings of compatriots the old-home dialects tended to emerge nostalgically from the semi-subconscious; but that is merely a retarding element, and even on these occasions the leveling of homeland subdialects took its course. While the educated generally leave dialects behind and speak a colloquial close to the written language, among the expellees there are some educated families which began to speak the old dialect in the new home although they had not used it before the expulsion. This is another retarding element without a future. It merely represents loyalty to memories of the lost homeland, which die with the older generations.

4.4. Example: South and North

The difference between south and north German dialects in their resistance to the inroads of the standard language is well documented in a statistically fortified study by Otto Steiner,[31] which takes both native and immigrant children into account. All school children in the first four grades (age 6–10) of the two districts Bamberg (northern Bavaria) and Northeim (Lower Saxony) were studied in 1954, their speech types being classified by their teachers. Steiner gives figures for eight categories of speech types including standard German, local dialect, imported dialect, and a number of transitional groups. Only his results for the major categories will be reproduced here. He finds that boys use the local dialect far more commonly than girls, who prefer High German (standard German). In both regions, for both sexes, the share of High German is much larger in the cities than in the adjoining rural districts. Sole use of imported dialects is represented by a mere 1% of all cases. Adding speech noticeably influenced by imported dialects still yields only 5.7 to 8.3%.

More important for us is the difference between south and north. In and around Bamberg 80% of the native pupils still speak the local East Franconian dialect, including more than 50% in the city. The pull of the local dialect is still so strong that roughly half the children of newcomers have adopted it or speak it along with High German. Few speak only the imported dialect, more speak both it and either the local dialect or High German. Those from Berlin, the Russian Zone, and northeast Germany (Pomerania and East Prussia), whose home speech was very different from that of Bamberg, are not yet so well assimilated as those from Silesia and

[31] Otto Steiner, "Hochdeutsch und Mundart bei Einheimischen und Neubürgern der Kreise Bamberg und Northeim im Jahre 1954; Ergebnisse einer Schulkindererhebung," *Phonetica* I (1957), 146–156.

the Sudeten area, who form about half the contingent and whose home speech is more closely related.

The situation in and around Northeim is strikingly different. The local Low German dialect is spoken by fewer than 10% of the pupils, including only 11% of pupils from native families. High German is spoken by 60%; 20% use High German and the local dialect. In the city more than half the school classes speak High German exclusively. The imported dialects of the newcomers, here prevailingly Silesians and northeast Germans, are spoken by very few of the children, and few of them have learned the local dialect. The Northeim statistics show clearly that these children contribute significantly to the strengthening of High German. The figures for Bamberg show the same effect, although there the position of High German has not yet become so strong.

If the suspicion should arise that the teachers, whose professional duty is to cultivate High German, might be biased in favor of it in the data they supplied, Steiner invalidates the objection by quoting comments of the collaborating teachers. In and around Northeim the children speak High German during the recess, while playing, and in the street.[32] Those who speak both High German and Low German or another dialect reserve the dialect for conversation with their parents and grandparents, while among themselves they speak High German, adorned with occasional dialect expressions. Steiner is sure that the "unexpectedly radical turning away from Low German" is a fact, and my own observations in other Low German areas confirm its rapid decline. Steiner expects the imported dialects to die out in a few years and the "dramatic change" in the relationship between High German and the local dialects to continue, and this remark does not refer only to Low German provinces.

Steiner's study is of great interest because it does not deal with big cities, where the decline of dialects and the progress of the standard language is

[32] Steiner's inquiry covered more than is contained in the published study. For example, he told me (St) that the teachers explored the play combinations of the various population elements by asking the pupils, "Who plays with whom?" The purpose of the question was not revealed to them; it was represented as a test to see whether they were good observers. The play groupings still showed complexes of strangeness in 1950. By 1954 the newcomers' children were integrated. Such sociological facts have of course a bearing on the children's speech. Individual differences also play a part. Gifted children are integrated more quickly than others because they are more popular as playmates, a fact which has been demonstrated statistically. The IQ distribution of the newcomers was about the same as that of the local population, as K. Val. Müller established statistically in 1946; the shocking disturbance of their environment had not affected it. For Müller's work, cf. in this connection especially: Karl Valentin Müller, *Heimatvertriebene Jugend*, Würzburg, second ed., 1956.

bound to be more rapid. My own observations were naturally made mostly in cities. Here however we have objective statistical evidence that the process goes on in small cities and in the country—at an amazing rate in the Low German speaking north central part of Germany, more slowly on the Franconian speaking northern edge of south Germany. Several typical aspects are confirmed by the study: the faster switch to High German in cities, even small ones, than in the country; the importance of the change of generations, children being ahead of the parents, who act as a retarding element; and the influence of evacuees, expellees, and refugees in strengthening the standard language.

5.1. Summary: Method of Presentation

I have chosen to reproduce the results of three exact studies rather than to register the numerous details which I collected myself during my travels through Germany. My own observations gave me an overall view of developments in many dialect areas, the strong impression that the dialects were receding at a surprisingly fast rate before the standard language (especially in the Low German north, but not only there), and the conviction that the strengthening of the standard language is the most important linguistic development in the German speaking lands today. I had not expected to find this as the salient feature of recent German.[33] My surprise provided the impetus for this report.

It is one thing, however, for the traveler to get a general impression based on a mosaic of a thousand details observed in contacts of daily life, and another to demonstrate the changes convincingly to the reader. I decided that the presentation of a few concrete, objectively analyzed situations was more suitable for this purpose. Besides, no one can, of course, have an intimate knowledge of all the native dialects of the German speaking lands in the west and all the dialects imported from the east. I had to leave the exact investigation of individual dialects to experts who concentrated their attention on limited sectors of the total situation. Even Langer, who restricts his study to expellees from his small home district in the Sudeten land, feels that the fate of their dialects in the various parts of West Germany is too comprehensive a topic for one man to investigate. Much less is it possible for one person to give the overall picture in exact detail.

[33] I have reported about many other features in my article (see note 17). The victory of "dialect-free High German" in a new expellee settlement in Hessen, in a mountainous wilderness near the Spessart forest, over the dialects of speakers from 13 different home regions is impressively shown in a popular article by Siegfried Mauermann, "Hält sich die Mundart der Heimatvertriebenen?" *Muttersprache* 1957, pp. 249–252.

It must suffice to state, on the basis of my experience, that the selected cases are representative of the whole situation.

5.2. Summary: Effect of Migrations on Dialects

The inroads made by the standard language into the domain of dialects, long in slow progress, have been greatly accelerated by the vast population movements since the war. The eastern German dialects are dying out, the western are receding. The decline of dialects amounts to a rout in the Low German north.[34] It is slower but also unmistakable in the south. The extent of the change is masked by the survival of speakers of the older generations who were reared with dialect speech as the natural medium of easy, informal, intimate contacts and do not change their practice abruptly when they are among themselves. But they are no longer among themselves all day and every day. The presence of numerous newcomers of different home speech in every city and every village makes them use their second language, standard German, much more frequently than before. The decisive step forward is linked with the change of generations.[35] Not long ago children liked to relax from school discipline by reverting to the unconstrained easy speech of the locality. Emotional values favored dialect, at least for children from families that did not cultivate High German at home. Even where they did, but High German was not yet completely natural, the speech of play would tend to emphasize features which were not favored by the home, to symbolize escape from restraint. It is striking to hear children in the street, all over the German speaking lands, now use more or less pure standard German among themselves, when the observer who remembers former conditions expects to hear dialect.

5.3. Summary: Leveling of Colloquial and Dialects

The struggle between standard speech and dialect concerns the spoken colloquial form of the standard much more than the written form. Few individuals speak as they write on all occasions. For the ordinary pursuits

[34] The amazing decline of Low German has not yet been recognized by the handbooks. In fact, Henzen (201) specifically exempts the Low German area from his statement that a colloquial close to the educated standard prevails more in northern cities than in southern. The situation is adequately described by Riemann (p. 29; see note 11), who speaks about the recession of Low German before High German among expellees from the northeast: "Was wir dort seit 70 Jahren registrieren konnten—nämlich das langsame Zurückweichen der Mundart der sozial Schwächeren vor der der sozial Überlegenen—, das vollzieht sich heute *in unvorstellbar schnellem Tempo* in den deutschen Westgebieten" (italics supplied). Cf. Moser, p. 138 (see note 5).

[35] The importance of the change of generations is stressed by Henzen (173f., 210f.).

of life most use a colloquial form of speech, in fact several forms depending on the nature of the contacts.[36] This colloquial speech still varies in different parts of the territory, depending on the strength of the regional dialect. It is typically a compromise between the standard language and the dialect. In the north the dialect elements are minor. In the south, particularly the southwest, dialect features in pronunciation and vocabulary are still much stronger but receding, and the provincial form of the colloquial speech tends to be uniform over larger areas than previously, the newcomers assisting in the leveling.

The leveling in the colloquial language is paralleled in the dialects. They used to vary from district to district, often from village to village, as the population units were largely self-contained. As the units were opened up by increasing intercommunication, the small subdialects combined into larger regional dialects with leveled-off features, at the same time as forms of standard speech made inroads into the dialects. Those among the newcomers who learned the new dialect learned it in leveled form and contributed to the leveling, strengthening the standard elements at the same time.

The development does not stop at political boundaries. Austria goes the same way as Germany. Switzerland, with fewer refugees and a linguistically very conservative attitude, applies strong brakes to the trend. The Swiss have no real colloquial between the standard language and the dialects. Even the educated speak colloquially a German which remains dialectal at least phonetically. But Switzerland, too, is slowly going in the same direction.[37]

6. Outlook

This seems to be the development in broad outline: even without the effect of the postwar migrations, small subdialects grow together into larger regional dialects. With powerful influence of the migrations, the dialects are affected more and more by the standard language. Along with

[36] The multiplicity of forms of the colloquial speech is emphasized by Henzen (20–25, 197–201), with a digest of the literature. Hugo Moser, in his inaugural address, *Mittlere Sprachschichten als Quellen der deutschen Hochsprache*, Nijmegen, 1955, makes the interesting attempt to prove that all forms of the German literary language, from Old High German and Old Saxon times on, were based on the colloquial speech of the circles which were successively the bearers of literature: nobility, clergy, monasteries, city patricians, and that leveling by regional compromises has always played a part in their development.

[37] Cf. note 12 above. See also Henzen (who is Swiss), pp. 191–193, for an interesting example of Swiss platform speech, which is really standard German with dialect sounds, and p. 193 for the progressing influence of the standard on Swiss dialect.

the modified dialect, a colloquial standard is spoken which has not yet shed the influence of the underlying dialect, but settles into forms unified over wide areas. These regional colloquial languages tend to reduce their dialect features and to adapt themselves progressively to a countrywide standard. The trend is toward a single colloquial standard over the whole territory.

The whole process—let it be said again—promises to progress radically with the change of generations. Children are ahead of their elders in the switch to the standard language. They contribute powerfully to the strengthening of the standard and to the decline of the dialects.

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K 2 id., interview 11 July 1955.

Kr Professor Eberhard Kranzmayer, Vienna, dialectologist. Native of Klagenfurt, Carinthia. Speaks Slovenian; has recorded the speech of the last Slovenian speakers around Klagenfurt. Interview 17 July 1955.

L Kurt Langer, expellee from the east Sudeten area, Czechoslovakian Silesia. Interview in Ennepetal, Ruhr district, 15 August 1955.

M W. Middelmann, Ministerialdirigent, Bundesministerium für Vertriebene, Bonn. Interview 14 April 1955.

S Schreiber, buyer at Stuttgart's leading textile firm (2000 employees and workers), where the occupational speech is still "purely Swabian." Native Alsatian, but for 24 years at present place of employment. Speaks Swabian at work with ease; spoke cultured standard German with me. Interview 27 April 1955.

St Otto Steiner, Brunswick, expellee from Silesia; mathematician and physicist with training in statistics; collaborator of K. Val. Müller, sociologist. Interview 19 May 1955.

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Criteria for Style Analysis

Abbreviations: *AR*='average reader', as defined in 2.2.2 (never used in the ordinary sense); *poem*='literary work of art' (cf. 0.2); *SD*='stylistic device'.

PROGRAM

0.1. Linguistics and stylistics. Subjective impressionism, normative rhetoric and premature aesthetic evaluation have long interfered with the development of stylistics as a science, especially as a science of literary styles. Because of the kinship between language and style, there is hope that linguistic methods can be used for the objective and exact description of the literary use of language. This, the most specialized and complex linguistic function, cannot be neglected by the linguists either.

Linguistic, structural description of style, however, requires a difficult adjustment: on the one hand, *stylistic facts can be apprehended only in language, since that is their vehicle;* on the other hand, *they must have a specific character, since otherwise they could not be distinguished from linguistic facts.* A purely linguistic analysis of a work of literature will yield only linguistic elements; it will describe those elements of a sequence which happen to have a stylistic value along with the neutral ones; it will isolate no more than their linguistic functions without discerning which of their features make them stylistic units as well. Still, the application of linguistic methods to such units would give us an objective knowledge of their double role as elements of both the linguistic and the stylistic systems. But this application requires a preliminary sorting. It is necessary to gather first all those elements which present stylistic features, and secondly, to subject to linguistic analysis only these, to the exclusion of all others (which are stylistically irrelevant). Then and only then will the confusion between style and language be avoided. For this sifting, preliminary to analysis, we must find specific criteria to delineate the distinctive features of style.[1]

[1] The dangers of applying linguistic methods to literary facts without a specific stylistic viewpoint were emphasized by A. G. Juilland in his review of Ch. Bruneau, *Histoire de la langue française*, vol. 13, in *Language* XXX (1954), 313–338. In this review Juilland clears the ground neatly but does not offer a practical solution; he does give useful bibliographical indications for the linguist's side of the question. On the stylistic

0.2. Scope. I shall try to outline some distinctive characteristics of the communication process in literature, and to take them into account when I introduce criteria relevant to style only. By *literary style* I mean any *written individual* form of *literary* intent, i.e. the style of an author or, preferably, of a single literary work of art (hereinafter called *poem*), or even of one isolable passage.

Style is understood as an emphasis (expressive, affective or aesthetic) added to the information conveyed by the linguistic structure, without alteration of meaning. Which is to say that language expresses and that style stresses, but the special values thus added to communication are not my concern; the consideration of these will come at the descriptive stage of stylistic research, or rather, at a metastylistic (e.g. aesthetic) stage. This paper limits itself to finding criteria and modes of existence of such values or stresses: its aims are exclusively *heuristic*.

0.3. Meaning of 'individual'. If stylistic phenomena occurred at random, without a code of reference, they would not be perceived. Even if style were only a metalinguistic fringe, there must be constants in its relationship to linguistic facts—without which constants style would collide with these

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approach proper, H. Hatzfeld's *Bibliografía crítica de la nueva estilística aplicada a las literaturas romanicas* (Madrid, 1955) is more complete than the English edition (1953); it is strongly biased in favor of Spitzer's methods (see *Romanic Review* XLVI [1955], 49–52). P. Guiraud's *La Stylistique* (Paris, 1954) is a survey of the main trends of style studies; so is Hatzfeld's "Methods of Stylistic Investigation," *Literature and Science: Proceedings of the 6th Congress of the International Federation for Modern Languages and Literatures* (Oxford, 1955), 44–51. These various works are primarily concerned with style studies in the Romance field, where they were systematized first. On the important Russian theoretical discussions of style analysis and its practical applications in the Slavic field, see V. Erlich, *Russian Formalism: History—Doctrine* (The Hague, 1955); the "Discussion of Problems of Stylistics" in *Voprosy jazykoznanija* 1954, no. 4, 76–100; A. B. Stepanov, "Problems of the Stylistics of Literary Usage in the *Učenye zapiski* and *Trudy* [of Soviet Universities], 1955–1957" (in Russian), *Voprosy jazykoznanija* 1958, no. 4, 108–115. Various American attempts have been made by A. A. Hill, S. Chatman, and others to adapt linguistic methods or techniques to stylistics. The fundamental difference between their approach and the one proposed here is that they do not introduce specific criteria; I shall devote a full discussion to their efforts in the near future.—In spite of its title, "Language and Style," S. Ullmann's introduction to his *Style in the French Novel* (Cambridge, 1957), 1–39, does not explore the nature of the interrelation of language and style. On the problem seen from the literary point of view, see R. A. Sayce, "Literature and Language," *Essays in Criticism* VII (1957), 119–133. Outside of the structuralist school, linguists side with the stylisticians and do not satisfactorily clarify the position of stylistics inside or outside of linguistics (see J. Marouzeau, "Langue et style," *Conférences de l'Institut de Linguistique de Paris*, 1945). The theoretical consequences to be drawn from the differentiation between language and style, and their methodological implications, remain a field largely unexplored.

facts: consequently, a general structure of style can probably be constructed.[2] But then, individual forms are to style what speech is to language. Studying them should give us basic data for a future system. I shall therefore exclude from the present essay all consideration of literary "dialectology":[3] as soon as elements from a literary language are used by an author for a definite effect, they become units of *his* style; and it is this particular realization of their value which is relevant, not their potential value in a standard system. If they are *not* used for a definite effect, all we can say is that they form for an individual style a background context more specialized than common speech would be—but they themselves are not style.

Now spoken individual styles are at best difficult to describe, easily stereotyped and therefore less differentiated from one another and from standard language than are written styles. Literary styles are complex, but for that very reason have features which permit clear differentiation. Hence I have chosen them for this heuristic experiment.

Distinctiveness of Linguistic Communication in Literature

1.0. The features I have just mentioned do not characterize every reduction of language to writing;[4] they are those which can be exploited by an author with literary aims (two-dimensionality, for instance, could be included; but it is used mostly in technical languages, and in literature it seems to have remained, so far, extra-linguistic, e.g. poetry with lines printed in the shape of the object described). I shall attempt to describe briefly what seems to me two distinctive features of literary communication, and then to show that these features can serve to elaborate some criteria of existence for the stylistic fact.

1.1. The encoder. The task of the author as encoder of the message is more exacting than that of the speaker. A speaker has to triumph over his addressee's inertia, absent-mindedness, divergent or hostile train of

[2] The possibility of a style structure has been questioned, e.g. by R. L. Wagner, *Supplément bibliographique à l'Introduction à la linguistique française* (Geneva, 1955), pp. 8–11.

[3] The difference between style and literary language was clearly seen by Yury Šerech, "Toward a Historical Dialectology: Its Delimitation of the History of Literary Language," *Orbis* III (1954), 43–57, but he centers his attention on literary language.

[4] E.g. the features defined by C. F. Hockett, *Course in Modern Linguistics* (New York, 1958), §62.5; for one thing, whenever a literary style aims at reproducing actual speech, it passes the boundaries of Hockett's *writing style*. Examples of literary two-dimensionality are to be seen in Mallarmé, "Un coup de dés," in Apollinaire, *Calligrammes*, in e.e. cummings' poetry, etc.

thought; he has to emphasize repeatedly, and this overdoing is concentrated on the most important points of the discourse.[5] But the writer has to do much more to get his message across, for he lacks linguistic or extra-linguistic means of expression (intonation, gestures, etc.), for which he must substitute underscoring devices (hyperbole, metaphor, unusual word order, etc.). Moreover, the speaker can adjust his speech to meet the addressee's needs and reactions; whereas the writer must anticipate all kinds of potential inattention or disagreement and give his devices a maximum efficiency valid for an unlimited number of addressees. And then there is the complexity of interplay between mere communication and the expressive, affective and aesthetic connotations (these latter exist in speech too, but the combinations cannot be so intricate as when built on paper at leisure; and the aesthetic intent is much less important).[6] The combination of connotations depends, naturally, on both the author's personality and his intentions.[7]

1.2. Control of the decoding. All this may sound self-evident, but I should now like to discuss how the encoder meets the challenge. Because the writer is obliged to be more forceful, has more to handle than the speaker, and has to write it down and later to correct it, he is *more conscious* of his message. The author's greater consciousness is not simply attention given to the encoding operation in order to make possible any prospective decoding: such attention is a necessary minimal condition for a written message anyway (e.g. in informal letters, in military orders: the addressee is expected to read them correctly, in his own interest, whereas the reader of novels may discard them, if not interested, without inconveniencing himself). The author's consciousness is his preoccupation with *the way he wants his message to be decoded*, so that not only its meaning but his attitude towards it is conveyed to the reader, and the reader is forced to

[5] For instance, wherever personal interest plays a role, as in the negation; good example of stylistic variety in H. Spitzbard, "Negationsverstärkungen im Englischen," *Die neueren Sprachen* II (1957), 73–83. On this "overdoing," see J. Marouzeau, *Traité de stylistique latine* (Paris, 1946), pp. 155ff.

[6] The founder of modern stylistics, Ch. Bally, limited style to "expression des faits de la sensibilité par le langage" (*Traité de stylistique*, 1902; ed. 1951, I, no. 19), but he excluded literary style. Recently, Herbert Seidler tried to limit literary style to the language's emotive potentials (*Gemüthaftigkeit*); see his *Allgemeine Stilistik* (Göttingen, 1953), pp. 62, 70, 76, etc.

[7] This is so obvious that there is no point in separating "two fundamental ways of looking at style," one regarding literary style as an individual form of expression bearing the stamp of personality, the other seeing it as "the means of formulating our thoughts with the maximum of effectiveness," as S. Ullmann still does (*Style in the French Novel*, Cambridge, 1957, pp. 2ff.) following P. Guiraud's *Stylistique* (Paris, 1954).

understand, naturally, but also to share the author's view of what is or is not important in his message.

This means going counter to the natural behavior of the receiver. We know that most of the time the transmission of a spoken chain is effected elliptically, for "the context and the situation permit us to disregard a high percentage of the features, phonemes and sequences in the incoming message without jeopardizing its comprehension."[8] This is just as true of a written text where the reader infers words from fragmentary spelling features, and reconstructs the whole of a sentence from the few words he actually perceives. Because the probability of occurrence in the written chain varies for different features, "it is possible, from a part of the sequence, to predict with greater or lesser accuracy the succeeding features." Under these circumstances, the decoding is bound to be erratic, more so than with a spoken chain, since a serious lapse of attention can always be compensated by rereading; the interpretations of the reader will be freer, and the intentions of the author may be foiled. If he wants to be sure they are respected, he will have to control the decoding by encoding, at the points he deems important along the written chain, features that will be inescapable, no matter how perfunctory the reception. And since predictability is what makes elliptic decoding sufficient for the reader, *inescapable elements will have to be unpredictable.*

In spoken transmission, the utterer can resort to the explicit clear-speech form, instead of a blurred form. This substitution is obviously not possible in writing, since conventional spelling and grammatical correctness have already subjected the decoder to explicitness.[9] The only procedure open to the encoder, when he wants to impose his own interpretation of his poem, is thus to prevent the reader from inferring or predicting any important feature. For predictability may result in superficial reading; unpredictability will compel attention: the intensity of reception will correspond to the intensity of the message.[10]

[8] Roman Jakobson and Morris Halle, *Fundamentals of Language* (The Hague, 1956), §1.5. I use *ellipsis* and *explicitness* in accordance with the description of the *Fundamentals*.

[9] Italics and other typographical devices may be a substitute for explicitness, but this is rare (except in technical writing): e.g. in English, italics representing affective stress are mostly used for *do*, *have*, possessive and personal pronouns.

[10] The stylistic cancellation of normal predictability creates expressivity by a process similar to what happens when we test speech intelligibility. An announcer utters isolated root words which a listener endeavors to recognize correctly, without any context or situation to aid him in the task of discrimination: the word, "deprived of any prompting context, either verbal or non-verbal, [. . .] can be recognized by the listener only through its sound-shape. Consequently, in this situation the speech sounds convey the maximum

Since such a control of the decoding is what differentiates expressivity from ordinary writing (which is indifferent to the mode of decoding provided decoding takes place), and since this differentiation corresponds to the complex of the author's message, we can see in it the *specific mechanism of individual style.*

It follows that an analysis of style based only on relevant features must concern itself with this fundamental mechanism. At the descriptive stage, it will be an application of information theory with special emphasis on the coding processes.[11] At the heuristic stage, our task is to gather the facts to be analyzed, namely the *elements which limit freedom of perception in the process of decoding.* If linguistic analysis cannot discriminate these elements from the irrelevant ones, it is because their potential is not realized in the physical body of the message, but in the receiver: thus the actual perception of the elements could be the criterion for their identification, and it seems that stylistics could use the reader as the listener is used in speech analysis.

1.3.0. Permanence of the message. Writing insures the physical survival of the literary message as the author conceived it; the patterns set for the control of decoding remain unchanged. But the decoders' linguistic frame of reference changes with the passing of time; the moment may even come when there is nothing left in common between the code to which the message refers and the code used by its readers. Meanwhile, the decoding of the poem's message reflects the extent to which the patterns of control suffer from the evolution of the code. This phenomenon has been neglected because its two faces (permanence of the code used in encoding and controlling; impermanence of the code used in decoding) have been considered separately; and this destroys its essence. The linguist, for instance, sees the poem as a mine of data for the reconstruction of a past state of the language. The stylistician tries to reconstitute the effects the poem's style had at the time of its creation, and to "correct" accordingly the reactions of modern readers. So much for the synchronic aspect; it is regarded from the viewpoint of the archaeologist. Others (critics and historians) devote themselves to retracing the evolution of readers' responses and how they confer upon the poem a value either ephemeral or eternal or renascent. These changes, however, are accounted for by the evolution of esthetic norms or by the

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amount of information" (Roman Jakobson, C. Gunnar M. Fant, Morris Halle, *Preliminaries to Speech Analysis*, Technical Report no. 13, MIT Acoustics Lab.; 2nd ed., 1955, p. 1).

[11] This may call for a modification of Joshua Whatmough's adaptation of the communication diagram to literature in his *Poetic, Scientific and Other Forms of Discourse* (New York, 1956), p. 219.

content of the poem. The essential role of form perception is hardly considered.

1.3.1. I submit that the specific viewpoint of stylistics should encompass this simultaneity of permanence and change. It should *combine synchrony and diachrony*, which is made possible by the dichotomy between encoder and decoder. On the one hand, a state of the language, in its structural integrity and its pre-set stylistic patterns of decoding control frozen by writing. On the other hand, actualizations of the poem's potentials, by successive generations of decoders, within the limits of the poem's patterns and the readers' codes, conflicting or not. The study of this combination will provide the history of the prolongation of effects despite the gradual vanishing of the code of reference for which they had been prepared. At the same time it will provide, for all synchronies considered, one after another, a description of the adjustment of two anachronic systems (the encoder's and that of any later decoder) maintained in synchronic contact. From the linguistic viewpoint, the problem is absolutely different from that of the conservatism of writing style in the face of changing speech habits. It has nothing to do with partial resistances to evolution, but with the degree to which and the manner in which an immutable system can continue working in changing sets of reference, with a widening chasm between the author's and the reader's codes (whereas, in the evolution of a normal language system, encoders and decoders continue to have a common code). From the stylistic viewpoint, we can find in the survival of SDs as efficient units an experimental confirmation of the expressivity revealed by other means of investigation.

1.3.2. The combination diachrony–synchrony appears in another form, this time within the poem: in the use of archaisms as SDs. The stylistic specificity of archaisms lies in this: that they suppose a special code (common to encoder and decoder) characterized by the consciousness of past states of the language.[12] When into a context given forms are introduced which belong to a state of the language anterior to the context, they

[12] See L. Flydal, "Remarques sur certains rapports entre le style et l'état de langue," *Norsk Tidsskrift for Sprogvidenskap* XVI (1952), 241–258, especially pp. 244–248 (against Saussure who denied that the speaker is conscious of the linguistic past, *Cours de linguistique générale*, Paris, ₄1949, pp. 117, 127; cf. Ch. Bally, *Traité de stylistique* I, no. 24). But Flydal weakens his demonstration when he extends the concept *archaism* to stereotyped phrases used normally in modern French (e.g. *à son corps défendant*), which are neither felt to be archaisms nor used stylistically as such. Furthermore, in order to justify his use of diachrony, Flydal treats archaisms as "incomplete systems" (*systèmes partiels*, p. 244) within the synchrony under investigation. He does this by shifting his definition from "parts of systems" (*partie des systèmes*) to "incomplete systems." This is a serious confusion. There are indeed complete archaic systems, extending to the

create a contrast which commands the attention of the reader. The archaism does function in the system of the context (and yet a synchronic description of the latter would overlook its archaic nature) but its function is twofold: as a linguistic unit it has its place reserved in the framework of simultaneous relationships; but as a stylistic unit it is decoded in reference to a second system, a past one whence the reader knows it comes (and yet a diachronic inquiry would overlook it as a functional element of the synchrony). The phenomenon of the archaism is certainly not novel—but the above reformulation has the advantage of having been done in stylistic terms.

Such reformulation will bring to light or permit explanation, by a few mechanic principles, of phenomena such as the following. The combination diachrony–synchrony which defines archaisms is contained in the poem's system and, physically, is as immutable as the other components of the system. However, integrated as it is with the exterior combination diachrony–synchrony, which represents the prolongation in time of the poem itself, the interior combination is modified by the exterior. As the unchanging poem grows chronologically older, i.e. is realized by readers further removed from its time of composition, the encoded divergence between the archaism and its context, and the resulting SD, disappear; the archaism and its context become an undifferentiated sequence which is felt to be either normal or archaic throughout.

Here again, during the life of the poem as well as at the time it was created, its actual perception by the reader, and the devices designed to insure it, are the essential characteristics of linguistic communication with literary intent.

Perception and Style Analysis

2.0. The stylistic type of communication defined above must now be located among the components of the written chain by means of criteria adapted to the nature of this type.

Since this communication is at the outset the author's response to an exceptional challenge, one would be tempted to start from his intentions and then to observe how they are realized in the text. But if the linguist has the relatively simple task of collecting all the features of speech of his in-

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whole of a poem (e.g. Balzac's *Contes drolatiques*, all written in "reconstituted" Middle French), but they are artificial. Moreover they are not a style but a kind of literary language, forming a background to style, a complete mimetic written standard (cf. 3.1.0 and footnote 24); such systems appear in fragmentary form, in historical novels, in the speech of the characters (e.g. *Kenilworth*).

formant without rejecting any, the stylistician must choose only those features which carry out the most conscious intentions of the author (which does not mean that the author's consciousness encompasses all the features). Now, it is often impossible to recognize these intentions except by analyzing the message, which makes the argument circular; or if they have become known by other means (philological enquiry, author's statement, etc.), they usually have a general character and cannot serve for the critique of specific passages. Finally, evaluating the realization of a writer's aims implies value judgments which belong to a metastylistic stage.

2.1. The use of the decoder in analysis. We have a better approach through the reader. For he is the consciously selected target of the author; the SD is so contrived that the reader cannot overlook it or even read without being guided by it to the essentials. Conversely the prolongation of stylistic effects in time as well as the perception of the poem at any given time depends entirely on the reader. This interdependence between the SD and its perception is, in short, so central to the problem that it seems to me we may use this perception to locate stylistic data in the literary discourse. Unfortunately taste changes and each reader has his prejudices. Our problem is to transform a fundamentally subjective reaction to style into an objective analytic tool, to find the constant (encoded potentialities) beneath the variety of judgments, in short to transform value judgments into judgments of existence. The way to do it is, I believe, simply to disregard totally the content of the value judgment and to treat the judgment *as a signal only*.

2.2. Analytic procedure. The procedural principle is the axiom "No smoke without fire." However right or wrong they may be, the value judgments of the reader are caused by some stimulus within the text. In the sender–receiver function which actualizes the poem, the receiver's behavior may be subjective and variable, but it has an objective invariable cause. In the linguistic message, more or less perceived, the passage from potential to actual style effect is a twofold phenomenon: first, the stylistic unit, and then, the aroused attention of the reader. It follows that stylistic investigation will have to use *informants*. These will provide us with reactions to the text: for instance, a native endowed with consciousness of the object language will read the text and the stylistician will draw from him his reactions. The segments of the text which cause his reactions, the informant will call in turn beautiful or unaesthetic, well or poorly written, expressive or flavorless; but the analyst will use these characterizations only as clues to the elements of the relevant structure. The analyst will not consider whether or not they are justified on the level of aesthetics. Furthermore,

contrary to Bloomfield's rules for the use of informants in linguistic investigation,[13] it would be better to have cultivated informants who are prone to use the text as a pretext for a show of learning: they will notice more facts in their eagerness to make many comments and exaggerate the difference between what they call style and what they consider more "normal" expression. Such secondary responses, misleading in linguistics,[14] and possibly elicited by ephemeral fashions or prejudiced aesthetic beliefs, will not harm the objectivity of the inquiry because, once again, they will be recorded only as signals of what causes them. So far style analysts have started from their own judgments of value; they have habitually confused them with stylistic facts, a confusion which stems from normative rhetoric; and they have rationalized on the basis of this confusion, supplying motives or labeling it common-sense evidence. Quite to the contrary, I hold that the stimulus referred to and which prompted the reaction is indeed present; but it must be considered independently of the reaction once the reaction has helped delimit it. *Stripped of its formulation in terms of value, the secondary response becomes an objective criterion for the existence of its stylistic stimulus.*[15]

2.2.1 The core of the procedure here proposed lies in separating clearly the psychological processes and cultural conditioning of the perception from its stimulus. This is enough to differentiate the procedure from Spitzer's,[16] or to substitute objectivity for his impressionism. Spitzer draws from one detail an inference as to the psyche of the author, which hypothesis is then controlled by a scrutiny of other striking details occurring in

[13] *Outline Guide for the Practical Study of Foreign Languages* (Baltimore, 1942), pp. 2–4.

[14] As Bloomfield showed during his controversy with Spitzer: L. Bloomfield, "Secondary and Tertiary Responses to Language," *Language* XX (1944), 45–55.

[15] I applied this criterion, without developing its theory, in my *Style des Pléiades de Gobineau; essai d'application d'une méthode stylistique* (mimeographed version, New York, 1955; printed revised ed., New York and Geneva, 1957). Hatzfeld (*Romanic Review* XLVIII [1957], p. 219) tried to assimilate my informant to Dámaso Alonso's reader-critic. He thus missed the essential point completely, i.e. that I disregard the reader's criticism as criticism. I observe it as a behavior. No matter how subjective the nature of a behavior (cf. G. Gougenheim, *Revue des sciences humaines*, no. 87, p. 344), its relation to a cause is an objective fact.

[16] In fact the present procedure develops and objectively formulates Spitzer's (cf. Juilland, *loc. cit.*, p. 321, footnote 7). See my "Réponse à M. Leo Spitzer: sur la méthode stylistique," *Modern Language Notes* LXXIII (1958), 474–480 (1958), especially, pp. 475–476 and 478. On Spitzer's method, see his *Linguistics and Literary History* (New York, 1948), 11–39, or his summary (with his reply to Wellek and Warren) in "Explication de texte," *Archivum linguisticum* III (1951), p. 1, footnote 1; also J. Hytier, *Romanic Review* XLI (1950), 42–59.

the same text. Thus Spitzer builds on the first clue which thrusts itself upon his attention, indeed, on his own interpretation of it. This interpretation in turn rests upon the assumption that there is a relationship between a given feature of the discourse and a state of mind. Thus we have an isolated point of departure for an all-encompassing construction, a strong temptation to be subjective. In the procedure I propose the analyst will scrupulously avoid hypotheses on facts thus designated and will wait, before building a structure, until all signals collected constrain him by their interplay and convergence to an interpretation taking them all into account. What is more alarming still in Spitzer's method, the purported control by other striking details may cause the analyst unconsciously to exclude clues which might have struck him were it not for his preconceived construction. Not only has the stylistic stimulus been confused with the value judgment, but the control, applied to the judgment instead of its source, is exercised upon the coherence of the analyst's response rather than upon the text as it should be.

2.2.2. Aspects of the informant. The preceding does not imply that Spitzer's reactions cannot be used when reduced to their stimuli. And for that matter, any previous commentary on style can be used with this double proviso: that such critical comments or value judgments or applications of systems must have been formulated about specific textual passages, and that we will disregard their meaning absolutely. This is especially important for poems of the past; for the stylistician will be able to locate possible SDs each time he finds in previous editions or bodies of criticism a footnote to the text: it does not matter that this note was intended to clarify for lay readers what was thought to be a grammatical difficulty, a poetic licence or an obscure meaning. All such responses, regardless of terminology or viewpoint, have stimuli; and these may be stylistic. Error and bias are unimportant: misinterpreting facts still points to facts. Even a denial of stylistic value at any one point may imply stylistic value. (This, contrariwise, cannot apply to linguistic description which, by definition, is total, defines all the discrete elements of the recorded sequences while not separating those which are actually perceived from those which are not.)

The analyst himself may play the role of informant; but obviously he will have more trouble separating his own secondary responses from their stimuli than with other informants, and the more so as he re-reads. One could conceive of an experimental translation into another language as a type of informant: an instance of "free" translation might warn us that there was at that point a SD which defied literal translation. However, structural differences between languages, as well as SDs, may prompt the

use of free translation; the problem would be further complicated by the second stylistic analysis on the translation.

These various types of informants will give us clues to the stimulus(i) encoded in the discourse. And for the stimulus(i) encoded in a poem of the past, the operation may be repeated for each generation of readers. The group of informants used for each stimulus or for a whole stylistic sequence will be called *Average Reader* (hereinafter AR).

2.2.3. The AR's metalanguage. Though we reject the AR's own interpretation of his reaction, it is useful to keep his technical terms, which constitute a fragmentary metalanguage borrowed mostly from the categories of rhetoric (metaphor, hyperbole, etc., or their common speech equivalents).[17] Such labels may be erroneous or not, like the value judgment which accompanies them, but they inform us about the linguistic consciousness of the AR's period in history and give us an idea of the effect of the SD's in relation to this Sprachgefühl: for instance, French Symbolist poets used a number of nouns in *-ance*, so many indeed that these nouns became a trademark of their style; for these poets these nouns were archaisms endowed with the quaint charm of things past. One of the most poetic was *assouvissance* and Flaubert made effective use of it; half a century later, when post-Symbolism had popularized this morpheme as a stylistic potential, Flaubert's archaism was reacted to and admired, now as a neologism.[18] The connotation of the word is not affected even if historical linguistics discovers that the element, foreign to the *état de langue* and interpreted as a neologism, is actually an archaism. Error for the linguist, fact for the stylistician. Metalinguistic distortions may serve as evidence of the diachronic extension of style effects (as outlined in 1.3.1).

2.3. Efficiency of the AR. Aside from the AR's ability to perceive the specific character of style, he is especially useful as a heuristic tool since his perception reproduces—indeed, it is the very process of—the phenomenon observed (decoding of the literary message), and it places the analyst in the decoder's own position without the latter's subjective liabilities. Instead of starting with an arbitrary, culture-conditioned, common-sense segmentation of the text or with an irrelevant (e.g. linguistic) one, the

[17] An extension of this notion to the use of traditional grammatical terms by the informant would be wrong. The stylistic effect often overlaps linguistic units. A grammatical term such as *adjective* may cover various SDs: metaphor, expressive word-order unit, rhythm etc. Even when style and grammar units do coincide, to start from grammar would be to run the risk of assigning a permanent stylistic value to a linguistic unit, no matter what the context.

[18] Alex. François, *La Désinence* -ance *dans le vocabulaire français* (Geneva, 1950), p. 36.

analyst is enabled to delimit the elements by which the author curbed the decoder's freedom (shallow reading, etc.) and increased the probability of perception. Thanks to the AR, the analyst can observe how the very efficiency of these SDs keeps them operative despite linguistic evolution. Thus we can fuse into one the study of style in its genesis and original context, and the study of its survival in time, with all that these teach us about successive synchronic time-slices in the language.

For style analysis proper, the AR should permit a directness and a rapidity impossible with other approaches: this is evident in the case of elusive facts, such as the emotive connotation of gender in Fr. *mer* (f.) or *océan* (m.). But take simpler cases such as the expressivity of a periphrasis, a very common SD, stylistically efficient because the reader feels it as an unusual substitute for a commonplace noun. For the linguist, the structure used for the periphrasis has nothing to distinguish it from the norm; only a complex computation of probabilities of occurrence could reveal anything exceptional there and even so could not guarantee that this something is perceived. Still the AR's reactions will pinpoint it at once.

Stylistic Context

3.0. Shortcomings of the AR criterion. However practical and objective it may be, the AR criterion suffers from a double limitation:

3.0.1. The variety of actual decoders may conceivably result in such a pulverization of the structure of the text that the resulting segmentation into stylistic units would be too complex to be interpreted. (In fact, even superficial observation of readers' opinions on books and a survey of published criticism on given poems indicate that the dangerous dispersion does not occur until the interpretative stage: once the widely conflicting value judgments are weeded out, we find they all sprang from relatively few points in the text.)

3.0.2. The AR's validity is limited to the state of the language he knows: his linguistic consciousness, which conditions his reactions, does not reach beyond a short span of time in the evolution of his language. Hence two categories of error. (1) Errors of addition: normal, hence irrelevant linguistic elements of a past state of the language look like stylistic units (e.g. archaisms), because they have disappeared from the language system used by the readers and therefore strike them as abnormal. (2) Errors of omission (which are, by the way, beyond the reach of linguistic analysis): relevant stylistic elements of the poem are in time assimilated into standard usage; they become and look like normal irrelevant speech units in a later

state of the language, ceasing accordingly to be noticeable to the readers; thus they lose their effect.

An example of omission is French *réussite* 'successful achievement', which does not produce any stylistic effect; no modern informant will be able to feel the effect it had in a 17th-century text on a 17th-century reader because *réussite* (as opposed to *heureux succès*, a synonym void of stylistic value) was a bold novelty: at that time everyone sensed it as a recent borrowing from Italian *riuscita*. An error of addition would occur, in turn, now that *réussite* has lost its pristine vigor and *succès* no longer needs the adjective *heureux* to be synonym of *réussite*. Consequently, *heureux succès* is felt as an archaism and is used now as an expressive equivalent of *réussite*. Yet the informant who gives it this expressive value in a 17th-century text is wrong, since it was then normal.

Such AR reactions are one of the interesting consequences of the poem's extension in time (see 1.3ff.). The errors of omission (obviously distinguished from the additions which never were style) are errors *only in the synchrony* represented by the poem at its birth: their identification concerns only the literary historian and the stylistician studying the constants of literary language of the time and their use as background style (see 0.3). But in the diachronic prolongation of that recorded synchrony, they are *not* errors, they are stylistic facts; they correspond to the invariable elements of the poem's frozen system, elements whose reference to the code varies with the modifications of this code, i.e. with the successive states of the language which, in turn, correspond to the successive generations of readers.

3.1.0. Irrelevance of the linguistic norm. The limitations outlined above make it imperative to add new criteria to complete and control AR's results. The SDs are, we remember, unpredictable for the decoder, against the background pattern of predictability offered by the frame of the language. Would it then be possible to discover the SD in any deviation from the linguistic norm? Most stylisticians believe so,[19] but it is difficult to see how we can use the deviation as a criterion, or even how we can describe it.

Against its use as a criterion, Juilland (*loc. cit.*, 1.6–1.7) raised strong

[19] L. Spitzer, *Linguistics and Literary History*, p. 11; J. Mukařovský, "Standard Language and Poetic Language," in *A Prague School Reader*, P. L. Garvin ed. (Washington, 1955) 19–35, especially pp. 20, 25, 31, 33; Wellek and Warren, *Theory of Literature* (Harvest Books, ed. 1956), pp. 166, 169, 171; R. A. Sayce, *Style in French Prose* (Oxford, 1953), pp. 1, 88, 131; Ch. Bruneau, "La Stylistique," *Romance Philology* V (1951–1952), 1–14, especially p. 6; P. Guiraud, "Stylistiques," *Neophilologus* XXXVIII (1954), 1–12, especially 7–8.

objections based chiefly on its limitation in scope: if we are to see style in the deviation from the norm, we will have to limit style to what is left of the written chain after eliminating every element which we can describe in its totality through linguistic analysis, and must therefore define as normal. But the answer is that we cannot then account for the fact that this very material we now reject may play a stylistic role when seen in other relationships.[20] And furthermore, the analysis will be warped in any case, because the departure from the norm is an ambiguous factor oscillating between consciousness (style) and automatism (unconscious mistake, mechanical imitation or self-imitation). (It will be noted that, of these weaknesses inherent in the criterion, the first could be obviated by the use of AR reactions, if this did not create a vicious circle.) Thus, if we were to use the linguistic norm at all, these limitations would dictate a complex, hardly practical set of precautions.

Now, to this use of the norm I see an objection which is more fundamental than those just mentioned, namely not so much that the linguistic norm is virtually unobtainable,[21] but that it is irrelevant. It is irrelevant because the readers base their judgments (and the authors their devices) not on an ideal norm, but on their individual concepts of what is the accepted norm (e.g. what the reader "would have said" in the author's place). These multiple norms are given some common traits by normative grammar.[22] Can we at least evaluate them with some approximation to accuracy? An overall norm, even for a short period of history and for a single social class, will not do, because even a relatively stable state of the language is the theater of transformations that style is likely to reflect.[23] Aside from the

[20] The sorting out of all stylistic features which I suggested in 0.1 does not have this disadvantage. I select elements only because of their actual value in a definite context; without the context, they are neutral and free to form any or no stylistic associations.

[21] As Juilland points out (*loc. cit.*, p. 318, footnote 5); he seems however to have hesitated before the radical conclusion: total rejection of the norm. To his arguments, of a logical nature, against the possibility of establishing a norm, another argument could be added, based on the importance of perception in matters of style: if the linguistic norm is presented in the form of frequencies and probabilities of occurrence, it must be recognized that a considerable gap exists between a divergence quantitatively expressed and the threshold of its actual perception by the reader; he seldom perceives any but wide divergences. There are deviations with stylistic effect, and others without.

[22] Which in turn is too conservative to be really used as a norm. It is true that some countries respect the grammarians' edicts more than others. In French style it is possible to discern the influence of a grammatical norm artificially imposed (see L. C. Harmer, *The French Language Today*, London, 1954, 11–46). This, however, is exceptional. German literature provides an example of a totally different situation.

[23] Cf. C. F. Hockett, "The Terminology of Historical Linguistics," *Studies in Linguistics* XII (1957), 57–73, especially 63–64.

author's milieu, we have to take into account his readings, his literary affiliations and probably a second standard, the written norm. This again will not suffice: modern literatures offer many instances of a third standard (I should call it the *mimetic written norm*), which is a very fragmentary system the writer uses to suggest substandard speech.[24] All these norms should be taken into account and our task should be done over for each generation of readers. Even if the notion of norm must be retained for the interpretative stage of stylistics, it cannot be used at the heuristic stage.

3.1.1. The context as a norm. At this stage, however, there is a way of avoiding the application to the text of a vague koiné or of a shifting Sprachgefühl: that is, by substituting the context for the norm.[25] Each SD, tentatively identified by the AR, has as its context a concrete, permanent background; one does not exist without the other. The hypothesis that context plays the role of the norm and that style is created by a deviation from it is a fruitful one. If in the style–norm relationship we understood the norm pole to be universal (as it would be in the case of the linguistic norm), we could not understand how a deviation might be a SD on some occasions and on others, not. For, if the variation from the general norm

[24] I do not have in mind consistent efforts towards real imitation but the conventional reproduction of a few typical forms of substandard speech, with spelling changes to indicate a given pronunciation (a good example of both imitation and mimetic written norm is G. B. Shaw's *Pygmalion*, Act I). Since the system is incomplete, the discrepancy between the narrative proper and the characters' speech is smaller; but this incompleteness follows a regular pattern, e.g. modern French conjugation, first person, and second person formal:

Substandard Speech	Mimetic Written Norm	Written Norm
[ʃsɛpɑ] [ʃʃɛpɑ]	*je sais pas*	*je ne sais pas*
[vusavɛpɑ]		*vous ne savez pas*

I am using examples from Bj. Ulvestad, *An Approach to Describing Usage of Language Variants* (Indiana University Publications in Anthropology and Linguistics 12, 1956), p. 43; he does not distinguish a mimetic written norm. I here correct his assumptions concerning French facts, which require the concept of a third norm. There is still another possibility: the creation of a mimetic norm by an author in order to particularize a character rather than a social class. It does not correspond to an accepted usage and could be termed *nonce-standard:* it would be interesting to see how far this can go without making the decoding impossible (a good example of nonce-standard is Shylock's speech: O. Jespersen, *Growth and Structure of the English Language*, London, ₉1938, §231).

[25] Used as another criterion, along with the AR method, this mechanical contrast with context should not leave much to the subjective Sprachgefühl (cf. L. Hjelmslev and H. Uldall, *Outline of Glossematics*, part I, Copenhagen, 1957, p. 3).

is constant, then the effect must be constant too. To cite an example, an unusual word-order such as, in Modern French, the Verb–Subject order (VS), when it is neither interrogative nor preceded by an adverb, is always abnormal and constitutes a constant deviation: we would expect it to be constantly expressive; yet it is *ceteris paribus* now a device for stressing the verb, now an indifferent sequence. Nor could we understand that a linguistic unit whose role is in certain relationships purely functional can become a SD elsewhere; nor again that a worn-out SD, having become a cliché without effect, can be rejuvenated and can regain expressivity on a background of ordinary speech; nor, finally, that the so-called 'naked styles' like Voltaire's, which seem never to depart from the simplest forms of everyday usage, could have any distinctiveness. The variability of effect of a constant anomaly is on the contrary immediately explained if the pole of opposition varies as well; and this pole varies by definition if it is the context. In the example cited above, the abnormal VS order (1) is linguistically constantly abnormal; (2) is expressive in a context where the preceding clauses present the normal SV order; (3) is no longer expressive if it appears in a context with a high frequency of VS order. Because of this frequency the reader's Sprachgefühl, which normally warns him of the VS anomaly, is dulled; the occurrence of one more VS unit becomes highly probable—in other words, the norm. There is no longer a style-creating contrast. Confirmation of this interpretation may be seen in the stylistic evolution of the last thirty years: the VS order has been so popular a SD that it tends to lose its expressivity in contrasting contexts, and it is now used for the most part as a conventional unit of the written standard.[26]

The application of the context as a criterion and as a corrective to the AR's shortcomings is easy: if there is no contextual contrast at the point where AR's reaction indicates the probable presence of a stylistic stimulus (that is, if the hypothetical SD and its context do not present any discrepancy, such as a difference of social level between words, entailing conflicting connotations, or a different word-order, or a change from parataxis to hypotaxis, etc.), then we can assume that there was on the part of the AR an over-response to the text (cf. 3.0.1) or an error by addition (cf. 3.0.2). In that case we may safely dismiss the initial clue.

3.2.1. Definition of the stylistic context. Since stylistic intensification results from the insertion of an unexpected element into a pattern, it supposes an effect of rupture which modifies the context. This brings out a

[26] On this typical example, see R. Le Bidois, *L'Inversion du sujet dans la prose contemporaine* (Paris, 1952), xvii+448 pp.; the facts are presented from the grammarian's viewpoint; this obscures at times the stylistic reality.

radical difference between 'context' in its common sense and stylistic context. A stylistic context is not associative, it is not the verbal context which reduces polysemy or adds a connotation to a word.[27] The stylistic context is a linguistic *pattern suddenly broken by an element which was unpredictable*, and the contrast resulting from this interference is the stylistic stimulus. The rupture must not be interpreted as a dissociating principle. The stylistic value of the contrast lies in the relationship it establishes between the two clashing elements; no effect would occur without their association in a sequence. In other words, the stylistic contrasts, like other useful oppositions in language, create a structure.[28] A consequence of the principle of contrast is that it enables us to draw the line easily between a personal style and a literary language: literary language in a poem needs no contrast; it is in fact a predictable pattern, whose linguistic elements receive no stylistic value from merely belonging to the language (e.g., Latin *letum* is the poetic-language equivalent of *mors* 'death' and is nothing more than the predictable substitute of *mors* in the poetic context); should the same elements appear in a context where no such conventional words are expected, they will be SDs (e.g., *letum* in familiar poetry has an ironical effect; Horace, *Sat.* 2,6,95, places it in the mouth of his philosophizing city-mouse).

3.2.2. Let us examine more closely the stylistic context. The patterning which conditions the reader for the "surprise" follows necessarily the progression of the sequences. The context could be represented as a linear segment oriented in the direction of the eye's progress in reading a line. Along this vector, information, forms, and the memory of preceding sequences will accumulate. The more clearly delineated the pattern is, the more effective the contrast will be (e.g., a narrative context with verbs in the past tense leading to the contrast of a single historical present; a series of rhetorical periodic sentences leading to the contrast of a sequence of short asyndetic nominal sentences).

Two corollaries to this definition can be formulated. First, the stylistic context has a narrowly finite extension, limited by the memory of what has just been read and by the perception of what is now being read; the context follows the reader, so to speak, covering all the sequences of the discourse. This explains the *polyvalence* of the SD, that is, the capacity for one device to give rise to many effects (e.g., a given word-order can stylistically affect any group of words where order is variable, regardless of the meaning

[27] See S. Ullmann, *Principles of Semantics* (Oxford, 2nd ed., 1957), pp. 60–63, 109.

[28] Cf. H. J. Pos, "La Notion d'opposition en linguistique," *Onzième Congrès International de Psychologie* (Paris, 1938), p. 245.

which is stressed).[29] Second, there can be an overlapping of stylistic units: if we define such units as Context+SD, it may happen that the SD establishes a new pattern and thus becomes the beginning of a context which is the first member of the next stylistic unit. For instance, aside from the type Context→SD→Return to context, we may have the type Context→SD starting new context→SD (e.g., for the first type, normal statement→ hyperbole→normal statement; for the second type, normal statement→ hyperbole→hyperbolic context→litotes). These occurrences imply that there is no such thing as an intrinsically stylistic device (e.g., an hyperbole in an hyperbolic context will pass unnoticed).

Borderline cases exist where the stylistic effect seems to stem less from contrast than from a cluster of SDs. The presence of such groups may become for the analyst a supplementary criterion.

3.2.3. Context and convergence. By stylistic clusters, I do not mean phenomena like the special case of phonetic expressivity where sounds seem to the reader to echo the meaning of words (*harmonie imitative*, *Lautmalerei*). The SD's effect supposes a combination of semantic and phonetic values; either one, separated from the other, would remain potential. I mean instead the accumulation at a given point of several independent SDs. Alone, each would be expressive in its own right. Together, each SD adds its expressivity to that of the others. In general, the effects of these SDs converge into one especially striking emphasis. For example, in Melville, *Moby Dick*, Chap. 51:

> And heaved and heaved, still unrestingly heaved the black sea, as if its vast tides were a conscience.

There is here an accumulation of (1) an unusual VS word-order; (2) the repetition of the verb; (3) the rhythm created by this ternary repetition (plus the combination of this phonetic device with the meaning: the rise and fall of the waves is "depicted" by the rhythm); (4) the intensive coordination (*and . . . and . . .*), reinforcing the rhythm; (5) a nonce word (*unrestingly*) which by its very nature will create a surprise in any context; (6) the metaphor emphasized by the unusual relationship of the concrete (*tides*) to the abstract (*conscience*) instead of the reverse. Such a heaping up of stylistic features working together I should like to call *convergence*.[30]

We have here a good example of the extent to which decoding can be

[29] On the mechanism of *polyvalence*, see my *Style des Pléiades*, p. 211 and passim; S. Ullmann, *Style in the French Novel*, pp. 9, 20.

[30] Convergence as a device was briefly noted by Marouzeau, *Traité de stylistique latine* (1946), 339–340, who saw its heuristic value, but did not suspect its importance in the "prolongation" of the poem's life.

controlled by the author. In the above instance it is difficult for the reader not to give his attention to each meaningful word. The decoding cannot take place on a minimal basis because the initial position of the verb is unpredictable in the normal English sentence, and so is its repetition. The repetition has a double role of its own, independent of its unpredictability: it creates the rhythm, and its total effect is similar to that of explicit speech (cf. 1.2). The postponement of the subject brings unpredictability to its maximum point; the reader must keep in mind the predicate before he is able to identify the subject. The "reversal" of the metaphor is still another example of contrast with the context. The reading speed is reduced by these hurdles, attention lingers on the representation, the stylistic effect is created.

We can find instances where the stylistic effect does not seem to imply any material context; in such instances, the convergence alone supplies the SD: it is the case for compressed poetic forms, such as the Japanese *hai-kai*, the prose poem (here *poem* is used in its common meaning), the monostich or one-line poem, and its corresponding prose form, the aphorism. In another case, the convergence is our only resource: the stylistic investigation of the beginning of any literary utterance. Here there is no patterning preparatory to contrast.

3.2.4. The convergence lowers the threshold of perceptibility of the SD, because of its cumulative nature; it may also function as a semantic context, limiting the polysemy of the word it stresses; thus the intentions of the author are made more clear. On the other hand the convergence is the only SD which we can describe with certainty as a conscious device; even if the convergence is first formed unconsciously or if it is accidental, it cannot escape the author's eye when he re-reads. Whether it be simply maintained or elaborated, it represents a case of extreme awareness in the use of language and may be the most complex stylistic form.

The usefulness of the convergence as a criterion is evident. Suppose a SD noted by the AR, but not presenting a clear-cut contrast with the preceding context: if it presents a convergence, its stylistic reality will be proven. Moreover, the convergence may give us an opportunity to correct the AR's errors of omission: even if there is no longer a common code, shared by author and reader, as is the case for medieval literatures, there is hope that analysis may identify, in the text, converging elements whose presence will reveal the SD which is no longer felt.

Indeed convergences may be the stylistic factor which insures the survival of the system encoded in the poem. If, in a convergence, successive generations of readers cease to perceive some of the component SDs because they no longer create contrasts in the new codes of reference (e.g.,

when a neologism has been assimilated to ordinary vocabulary; when a metaphor has become a cliché of the standard literary language, etc.), there is a chance that at least one of the components will be preserved as a stimulus of expressivity and will continue to represent the complex of decoding-control devices which had been placed there by the author.

Conclusion

To make stylistics a science, or to delimit that area of linguistics which will treat the literary use of language, it is not sufficient to begin from a subjective apprehension of the elements of style. It therefore seemed obvious that a heuristic stage should precede any attempt at description. The criteria presented here should permit objective determination of the facts: as criteria, context and convergence require only the observation of forms without prejudgment of their content or value; with the AR, value judgments are utilized only insofar as they reveal stimuli.

The criteria are specific because they correspond to the distinctive features of the poem. I have attempted to describe these literary features as a special case of linguistic communication, characterized by a modification of the coding processes and by the permanence of the literary message. If this reformulation is found useful, it will be because it tries to account for facts economically, without artificial separation of language and style. Once stylistic facts have been identified, linguistic analysis, applied to them only, will be relevant; prior to their identification, it cannot alone isolate them.

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Obituary

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In the course of a recent report on some highlights of the Eighth International Congress of Linguists, a distinguished Hungarian specialist in Finno-Ugric languages makes a bitter charge. "It is common knowledge," he laments, "that no Hungarian contributions to the theory of structuralism exist."[1] This sweeping indictment—belied a dozen pages later by the obituary of Gyula Laziczius,[2] an internationally appreciated contributor to the structuralist movement, but a prophet with hardly a follower in his own land—may well stand as an epitaph in memory of this dedicated linguist, whose career was shaped by a series of harsh ironies.

Laziczius was born on the 18th of August, 1896, in Újpest, a district of the capital where he died on the 4th of August, 1957. During his life of three score years, he was able to function barely a decade and a half as a productive scholar, so many and so oppressive were the reverses visited upon him. Having completed his elementary and secondary schooling in Újpest, he enrolled at the University of Budapest, in 1914; but his studies were interrupted by World War I, which he spent partly in a Russian military prison. Upon his return, in 1918, he continued his higher education and, after graduating in 1920, earned his living as a teacher, for four years, in a variety of secondary schools. In 1922, however, he was dismissed from his post, for political reasons, and ten years were to pass before he was able to free himself from stultifying clerical jobs and, in 1932, resume teaching. The following year, he completed a thesis for his "habilitation": "General Phonology, With Particular Reference to Hungarian and the Slavic Languages." In 1935, he became a corresponding member of the Academy; next year, an adjunct professor of Hungarian linguistics; then, in 1938, he

[1] Gy. Lakó, *Nyelvtudományi Közlemények* [Linguistic Communications] LX (1958), 197.

[2] By J. Harmatta, *ibid.* 210–213; a German version appeared in the Hungarian periodical *Acta Linguistica* VII (1958), 211–216. I am indebted to Harmatta's account for the chronology of the principal events in Laziczius' biography, and to J. Lotz for his helpful comments.

was called to occupy, as an extraordinary professor, the newly established chair of General Linguistics and Phonetics (which was, incidentally, the first structural academic position to be established anywhere), and became an ordinary professor of these subjects in 1940. During World War II, he suffered a series of tragic personal misfortunes, including the loss of his son, under particularly grim circumstances. In 1949, he was forcibly retired from his chair, once again for political reasons. He spent the eight remaining years of his life with hardly any opportunity for conducting scientific work, and in penury, scarcely managing to make a living as a hack translator. His heart condition of old deteriorated steadily after his discharge, but he survived for a few more years thanks to the medicines supplied to him by a famous American colleague.

In the 1920's, while Laziczius did office work by day, he gave his free time to the area of scholarship which first engaged his attention: Russian literature and philosophy. His doctoral dissertation, presented in 1929, dealt with "Belinskij and Hegel"; and he also published a series of articles on Tolstoy, Dostoevski, and other topics drawn from 19th century Russian thought. His maturation as a linguist coincided with the formative period of structuralism, the mid-twenties, when members of the Prague School as well as some of their American colleagues began to emerge from their chrysalis stage.

His first important article, "On Phonology," appeared in 1930, followed shortly by "The Phonology of a Hungarian Consonant Change."[3] These papers showed the direct influence, in the first place, of de Saussure; of the "Prague" linguists, notably Trubetzkoy and Jakobson; and, not least, of Sapir, whose "Sound Patterns in Language" made a considerable impression on Laziczius ("I believe that Edward Sapir was the first to introduce phonemic considerations in the study of sound changes," was the opening sentence of the second of his articles cited). Phonemics, as Laziczius conceived of it at the time, constituted an independent branch of descriptive linguistics, devoted to the study of the function of speech sounds—this formulation allied him most closely with the Prague point of view, which Mathesius had designated "functional linguistics"—and he undertook to state its three principal goals: to identify the sounds which possess either a lexical or a grammatical function in a given language; to decide how such elements are distributed in this or that language; and, most importantly, to examine and compare phonemic systems, i.e., to study typology. Unless phonemics leads to the formulation of comprehensive generalizations, it

[3] In *Magyar Nyelv* [Hungarian Language] XXVI, 18–30 and 266–276, respectively. For the purposes of this obituary, the terms "phonology" and "phonemics" may be taken as roughly equivalent.

will have failed, he insists, since individual events, no matter how interesting by themselves, have nothing to do with science. It is characteristic of Laziczius' procedure that he promptly attempted to apply the theory he displayed to a concrete corpus, and he showed how the phonemic point of view contributed to the solution of a well-known problem, that of the disappearance of word-final short vowels in old Hungarian.

Laziczius was deeply aware of his predecessors and contemporaries in the structuralist movement (Bühler and Twaddell were among those from whom he profited in later years). Yet he was by no means content to remain merely a disciple: "bien que j'aie toujours approuvé l'idée fondamentale de la phonologie, je considérais avec un certain scepticisme les enseignements qui se cristallisaient peu à peu autour de ce fonds commun."[4] Though he eventually became an innovator in phoneme theory, the first of his major works, *Introduction to Phonology*,[5] gives less evidence of this than of his remarkable erudition, his grasp of essentials, the lucidity of his formulation, and his versatility in applying a new technique in a field excessively burdened with tradition. Like his other two books, which came out in 1942 and 1944, this, too, was published only in Hungarian, a fact which, of course, prevented the instant international recognition it would surely have been accorded had it been written in a more accessible language. It is, indeed, one of the classical expositions of the field which, even today, repays careful reading.

The book is divided into three sections, roughly equal in length: (1) general phonemics, (2) Hungarian phonemics, and (3) historical phonemics. Among the most valuable chapters in the first part—which is otherwise vintage "Prague"—is a sketch of the history of phoneme theory, from the days of the Kazan' school of Baudouin de Courtenay and Kruševskij through the First International Congress of Linguists (1928); it is plain that the author had complete and critical mastery of the entire literature of his subject.

The second part contains not only the earliest—and in many respects still the best—description of the phonemes of Hungarian, but it also constitutes an original and brilliant exposition of structural dialectology, far ahead of its time. Laziczius succeeded in reducing to a simple and clear structure the apparently chaotic phonetic diversity which separates Hungary's eight dialect regions, and in relating this to the system of the literary and spoken standards; whether or not a structural dialectology is,

[4] "Phonétique et phonologie," *Lingua* I (1948), 294. It strikes me as a bizarre oversimplification to bracket Laziczius with A. Martinet as "adherents" of Trubetzkoy's, as W. K. Matthews does in *Lingua* VII (1958), 264.

[5] *Bevezetés fonológiába*, Budapest, 1932.

a priori, possible did not concern him, for he was perhaps the first linguist to resolve the question to his own satisfaction and to that of the few who possessed the equipment to grasp his argument.

It is in the third part of the monograph, however, that Laziczius breaks most decisively with the atomism of the Neogrammarians, and boldly outlines the principles of historical phonemic analysis.[6] "Sound changes in language always and without exception occur as mutations," he declares emphatically, and goes on to elucidate, with the aid of this novel instrument, several key problems of Hungarian linguistics. His final chapter, on the "Interdependence of Sound Changes," is a plea for intertwining the threads of isolated diachronic events, for searching out those "developmental tendencies" which characterize the particular state of a language, and for recognizing those internal pressures which condition linguistic changes.

Laziczius' later departures from phoneme theory as developed by Trubetzkoy centered around three problem areas: the definition of the phoneme in terms of psychological notions; the nature of phonetics; and the relationship of phoneme to allophone.

In regard to the first point, he in effect joined with most American linguists in sharply rejecting Trubetzkoy's use of "undefined psychological terms,"[7] but expressed himself as favoring the sociological approach of certain other Russian linguists, notably Vinokur and Šor. He also repudiated Trubetzkoy's conception of phonetics as a natural science. This view, he argued, is not borne out by either the history of or current practice in the field: "Sans considérer certains buts pratiques (enseignement des sourds-muets, etc.), on l'a toujours mise au service de la linguistique."[8] The gap between the two subjects—phonetics and phonemics—is not one of principle; it is, rather, an artificially induced breach motivated by academic politics, one which must be bridged, he argues, by a return to the tradition of Sweet, namely, the reintroduction into phonetics of the functional techniques of phonemic analysis: "les phonéticiens eux-mêmes se sont rendu compte à plusieurs reprises des différences fonctionelles qu'il y a entre les sons d'une langue donnée. Même s'ils n'avaient pas fait cette découverte, on ne devait pas leur refuser le droit d'en tirer profit. Pourquoi réserver cette distinction à la phonologie? Dans la science," he winds up on a characteristic note, "il n'y pas de monopoles."[9]

[6] A. G. Juilland's "A Bibliography of Diachronic Phonemics," *Word* IX (1953), 198–208, needs to be emended in regard to Uralic linguistics in general, and the "important explicative contributions" of Laziczius in particular.

[7] E.g., Z. S. Harris, in *Language* XVII (1941), 345–349. But in the publications of his later years, Trubetzkoy minimized, if not actually discarded, the psychological slant of his definitions.

[8] "Phonétique et phonologie," 298.

[9] *Ibid.*, 302.

It was Laziczius' view of the nature of the phoneme, however, and the ensuing amiable controversy between him and Trubetzkoy, for which he became most famous internationally.[10] Bühler's well-known model, dividing the act of speech into three aspects, furnished the philosophical peg upon which Laziczius hung his linguistic garment. These aspects were, it will be recalled, those features of sound which refer to the designation (*Darstellung*), those which are characteristic of the source (*Kundgabe*), and those which constitute the appeal to the destination (*Appell*). Laziczius' first point was that phonemes function in all three aspects and not just referentially; his second point was that, among linguistic signs, one encounters, beside phonemes and allophones, also another category, which embraces those elements which he dubbed *emphatics*—we would call them expressive features—which function only in reference to speaker and hearer but not the content; and, third, that the allophones function only in reference to the speaker but not either the hearer or the content. All three—phoneme, emphatic, and variant—were, moreover, Laziczius insisted, to be regarded as equally conventionalized. He was the first one, followed by van Wijk and Malmberg, to point out that the claimed proportional relationships, *phonology* to *langue* equals *phonetics* to *parole*, is incorrect, since the allophones are also socially determined.

While Trubetzkoy admitted the unilateral nature of phonemic investigations, he was of the opinion that data concerning expressive elements were so meager and unreliable that one could but speculate about their role in language, and he ended by relegating them, together with the features functioning in reference to the speaker, to a special discipline, namely, *phonologische Stilistik*. Laziczius, in his several replies, defended and elaborated upon his position, incidentally furnishing many novel data in its support; and he continued to accuse Trubetzkoy and his followers of a number of "errors," chief among them being their confusion of expressive elements and allophones.

Whatever one may think of this exchange, or of the merits of Laziczius' theory, the fact remains that he was the first to describe in functional terms (however gropingly perhaps) the relationship of what some would now

[10] These ideas were worked out, in several somewhat differing forms, in Hungarian: "Jeltan, elemtan," *Nyelvtudományi közlemények* XLIX (1935), 172–189, and also in *Általános nyelvészet* (General Linguistics; 1942); in German: "Probleme der Phonologie, Zeichenlehre—Elementenlehre," *Ungarische Jahrbücher* XV (1935), 193–208; in English: "A New Category in Phonology," *Proceedings of the 2d International Congress of Phonetic Sciences* (Cambridge, 1936), 57–60; in French: "Phonétique et phonologie," 298–300. For Trubetzkoy's counter-statement, see "Phonologie und Lautstilistik," *Grundzüge der Phonologie*, Prague, 1939, 17–29.

prefer to think of as distinctive, redundant, and expressive features. Realizing their importance only lately, linguists and psychologists alike, in collaboration and individually, have begun attempts to disentangle afresh, and with the aid of the newest tools of our trade, these interwoven threads that make up the total ribbon of communication.[11]

The appearance of his *Introduction to Phonology* ushered in Laziczius' most productive period: over the next twelve years, he published many papers dealing with all sorts of problems in Hungarian, Finno-Ugric, and general linguistics. Some appeared in leading periodicals abroad, commanded esteem, and brought him invitations to lend his name to the masthead of a number of journals, notably, *Acta Linguistica*, *Archiv für die gesamte Phonetik*, and *Zeitschrift für Phonetik und allgemeine Sprachwissenschaft*. He also came to play an increasingly authoritative role in international congresses of both phoneticians and linguists. At home, he was elected Secretary of the Linguistic Society of Hungary, and to full membership in the Academy.

Laziczius' researches were by no means restricted to language in its phonic aspect, for his articles touched as well on many different topics in morphology and lexicology, synchronic and diachronic. Writing "On the Question of the Formation of Finno-Ugric Tenses,"[12] for example, he introduces and discusses the notion of morphological zero (which must be treated exactly as we handle "material data, but naturally only within the system, when a corresponding opposition is available"); and proceeds to a fascinating and learned psycholinguistic excursus against the so-called "axiom of parallelism," that fuzzy, pseudo-Whorfian concept, which still has a remarkable number of adherents—the one about the color-blind Chinese (can they distinguish blue from green?). In "The Size of the Hungarian Lexicon,"[13] he makes the point that a quantitative approach to linguistic materials serves no useful purpose if the units counted were not established according to sound principles. "La Définition du mot,"[14] the last paper he published abroad, was composed during the painful war years; therein, he evaluates several definitions of the word and adds yet another: words are linguistic signs composed of specific phonic elements

[11] See, for example, Chapter 4 in C. E. Osgood and T. A. Sebeok, *Psycholinguistics* (Baltimore, 1954); E. Stankiewicz, "Expressive Language," work paper for the Conference on Style (Bloomington, 1958); G. L. Trager, "Paralanguage: A First Approximation," *Studies in Linguistics* XIII (1958), 1–12; R. E. Pittenger, "Linguistic Analysis of Tone of Voice in Communication of Affect," *Psychiatric Research Reports* VIII (1958), 41–54.

[12] In Hungarian; *Magyar Nyelv* XXIX (1933), 18–25.

[13] In Hungarian; *ibid.* XXXVIII (1942), 65–73.

[14] *Cahiers Ferdinand de Saussure* V (1945), 32–37.

capable of functioning in a linguistic context (*Zeichenfeld*) as well as in a non-linguistic environment (*Zeigfeld*), and constituting, at a particular moment and in a particular speech community, a system.

We have mentioned Laziczius' friendly debate with Trubetzkoy; but we must add that the art of dialectic was the method he favored most for the exposition of his views—and he excelled at it. He participated in many disputes, reasoning always with awesome cogency but often with perhaps needlessly offensive acrimony, earning himself a host of enemies. Typically, his papers open with a reaction to someone else's thesis, which he proceeds to demolish by argument supported with both data and a prodigious knowledge of the relevant literature; his antithesis is then stated and applied to some concrete linguistic problem which he elucidates in a higher synthesis. Once, when he was charged by a colleague with malice aforethought, he replied: "In my opinion, in science there is no such thing as an attack; there are only theories and hypotheses, data and proof, and he whose trump is the higher is right. . . . Attack is not my meat, but criticism is my right, indeed my duty."[15] And his attitude is further indicated by an observation he made "On the Margins of an Article": "Every piece of scientific writing, save when it touches on virgin territory, is more or less contentious in character."[16] In his passionate search for the truth, he was prepared to tilt with anyone, from distinguished foreign colleagues like Trubetzkoy or Bühler,[17] to Hungarian notables like Zoltán Kodály.[18]

The seeming multiplicity of Laziczius' linguistic activities was at last reduced and incorporated into a single, overarching theory of language, which he presented in the second of his major works: *General Linguistics—Questions of Principle and Technique*.[19] The twelve chapter headings may impart some of the flavor of the book: (1) The Autonomy of Linguistics; (2) The Subject Matter of Linguistics; (3) Bühler's Third Axiom—The Place of Linguistics; (4) Language as Sign Event; (5) Language as System; (6) Subsystems in Language; (7) The Unity of Language; (8) The Distinction Between Language and Speech as a Methodological Principle;

[15] *Magyar Nyelv* XXXI (1935), 48.

[16] *Ibid.* XXXVI (1940), 81–89. A part of this article is devoted to Twaddell's "splendid book," *On Defining the Phoneme;* while generally in agreement with him, Laziczius expresses some reservations in regard to Twaddell's doubts as to some views of the physical reality of the phoneme, certain ones of which—notably Bloomfield's—he regarded as soundly based.

[17] Cf. "Das sog. dritte Axiom der Sprachwissenschaft," *Acta Linguistica* (Copenhagen) I (1939), 162–167.

[18] "On Hungarian Pronunciation" (in Hungarian), *Magyar Nyelv* XXXIV (1938), 307–316.

[19] *Általános nyelvészet*, Budapest, 1942.

(9) Sign Event as a Methodological Principle; (10) Expression of Meaning and Expression of Emotion; (11) The Distinction Between Synchrony and Diachrony; and (12) Other Errors of Method.

Laziczius thought of this monograph as only a prolegomenon to his grand design: by clearing away the underbrush of methodological misconceptions, he wished to prepare the ground for a four-volume work, to be devoted, respectively, to phonetics, phonemics, lexicology, and syntax. Unfortunately, he was able to complete only the first of these, *Phonetics*,[20] although a version of the book on the morpheme and fragments of the one on syntax also survive. (He died, in fact, while he was completing a chapter on "The Central Problem of Syntax.")

Phonetics has already been reviewed in the United States in some detail.[21] It is a fine, mature, and, in some respects, unique book, one which pays equal attention to the genetic and the acoustic stages of the speech event; accords special treatment to each of the prosodic phenomena of duration, stress, and tone; and includes meticulous and critical accounts of the historic controversies around the diphthong and the syllable. In discussing the chapter on acoustics, the reviewer in *Language* had noted that, in consequence of wartime isolation, "the author had no access to the latest research in this field, which . . . has progressed enormously during the last few years in the United States especially." By the summer of 1948, when Laziczius had met and entertained the author of this necrology, he appeared to have read and thoroughly absorbed all that had been published on "visible speech," and he volunteered that this new literature had given him no cause to modify his theories in any fundamental way.

In Laziczius, we see the confluence of the best in Russian and American, Swiss, Czech, and Danish linguistic thought, which he enriched from his own nation's resources. It is not likely that, in our age, another general linguist of his stature will appear in Hungary. Laziczius wrote his major works in Hungarian, but he was a scientist in the main stream of modern linguistics, a river which, like language itself, overflows the boundaries of nations, and will not stay confined to continents.

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[20] *Fonétika*, Budapest, 1944. A few months before he died, Laziczius finished editing a German translation of his book, at the invitation of the Academy Publishing House of Berlin. We may presume that this translation, done by W. Steinitz, at the Hungarian Institute of the University of Stockholm, in 1944, will soon appear.

[21] *Language* XXIII (1947), 75f.

Discussion

"LINEAR CONTEXTS," LINGUISTICS AND MACHINE TRANSLATION

DEAN STODDARD WORTH

Many Russian words which are morphologically short form neuter adjectives can function syntactically as either predicate adjective modifiers of neuter substantives and a few other forms or as adverbial modifiers of verbs, adjectives, and adverbs (*ego postuplenie—zakonno* 'his enrollment is legal' :: *on postupil zakonno* 'he enrolled legally'); the same situation obtains with the comparative of adjectives and adverbs. This lack of an exact correspondence between morphologic form and syntactic function led the authors of a recent article in this journal[1] to try to analyze the syntactic function of these ambiguous forms on the basis of what they call "linear contexts," namely by looking only at the word or punctuation mark before and the word or punctuation mark after the form in question. The purpose of this analysis was to provide unambiguous instructions for a digital computer engaged in the mechanical translation of Russian, and did in fact provide "a few simple rules, which, in the majority of cases, prove sufficient to determine the syntactic function of these forms in a maximum environment of one word before and one word after the ambiguous form(s)" (p. 406). At first glance, this seems like an impressive accomplishment.

The article offers a set of "preliminary rules" which divide up several hundred occurrences of ambiguous forms into three major groups, two of which are classified on the basis of function ("I. Predicative Complement," "II. Adverbial Modifiers") and the third on the basis of form ("III. Sequence of two *-o* or one *-o*, one *-e*/*-ee*"[2]); these major groups are divided into twelve, seven, and three groups respectively, purely on the basis of what type of word or punctuation mark precedes and what type follows the form in question, for example "2. a) not verb + *-e*/*-ee* + *čem*," illustrated

[1] Andreas Koutsoudas and Assya Humecky, "Ambiguity of Syntactic Function Resolved by Linear Context," *Word* XIII (1957), 403–414.

[2] By *-o*, *-e*, *-ee* are meant positive or comparative adjective/adverb forms such as *postojanno* 'constant(ly)', *niže* 'lower', *bolee* 'more'.

by *gde temperatura niže, čem* 'where the temperature is lower than'.[3] However, the validity of these "rules" seems to be called into question by the fact that for almost all of these groups one can quite easily produce examples which satisfy completely the formal environmental requirements, but in which the ambiguous form nonetheless plays an entirely different syntactic role. Let us consider some concrete cases, first in major group I, where the ambiguous form is supposed to function only as a predicate complement.

In group I.1, "nonverb + *-e/-ee* + Adjective Class Member,"[4] the authors have by a regrettable oversight analyzed the wrong words. In their examples, *metallax krajne zatrudnitel'no* 'in metals is extremely difficult' and *naxoždenie ešče bolee zatrudnitel'no* 'finding is still more difficult', the central *-e/-ee* forms are of course not predicate complements but adverbs, whereas the forms *zatrudnitel'no* 'is . . . difficult', italicized in the original, are of course part of the environment, not the central forms.[5] In the remaining groups no further lapsus of this sort occurs.[6]

Group 2a, "nonverb + *-e/-ee* + *čem*" is illustrated by *gde temperatura niže, čem* 'where the temperature is lower than', but ignores such appositive constructions as (*vtoraja*) *temperatura, niže čem* (*pervaja*) 'the second

[3] In the present remarks all Russian forms are given in italics and the translation thereof in single quotes; this is a simplification of the system in the original article.

[4] "Adjective Class Members" (ACM) are defined as adjectives and participles, as opposed to non-participial adjectives alone (p. 407, footnote).

[5] Such phrases would seem to belong in major group II, "Adverbial Modifiers"; the nearest this major group comes to a group "nonverb + *-e/-ee* + ACM," however, is II.6, "irrelevant + *-o* + ACM."

[6] One regrets a number of careless errors in this article, some of which may have been beyond the authors' control. Most Russian words are given in the standard scholarly transliteration, but one is puzzled by the forms *n'et* and *'est* (each thrice, pp. 405–06), which seem an odd compromise between transliteration (*net, est'*) and phonemic transcription (/n'et/, /jest'/). Russian "è oborotnoe" is usually transliterated *è*, but in this article it appears as *é* (*poétomu, éto, éffektu* p. 408, *étoj* etc. p. 409), *ė* (*ėto, ėnergii* p. 408, *'to* p. 412), or plain *e* (*experimental'no* [sic] p. 411). The eighth letter of the Russian alphabet can be rendered as either *ž or zh*, but not *žh* (*Žhurnal* p. 406, footnote). In a footnote on p. 405 there is a curious equation of "flectional morpheme" with "suffix" and of "derivational morpheme" with "ending." In an article making quite extensive use of specialized terms such as "morphology," "flectional morpheme," "morphophonemic circumstances," one is surprised to read (p. 407) that the word *zakonno* has the suffix *-nno*, whereas *ravno* has the suffix *-no*; surely one can compare, if nothing else, the spelling forms *zakonno* : *zakonen* :: *ravno* : *raven* and arrive at the fact that *zakonno* = *zakon* + *no* just as *ravno* = *rav* + *no* (quite apart from the question of accurate morphophonemics). Such details, while of no great importance in themselves, are disconcerting in an article which makes rather broad claims of discovery in a complicated part of Russian linguistic structure.

temperature, lower than the first', where the *-e* form, although an adjective, is not a predicate complement and cannot be translated by 'is';[7] cf. also (*Ivan govoril*) *tonom niže, čem* (*Petr*) 'Ivan spoke a tone lower than Peter', where *niže* is closer to an adverb than to any other traditional part of speech, or (*temperatura padaet v takom*) *slučae niže, čem* (*v pervyj raz*) 'the temperature drops in such a case lower, than the first time', where *niže* is an adverb. Similarly, in group 3, "nonverb, nor *esli*+*-o/-e/-ee*+infinitive" we are given the illustrations , *legko polučit'* ', it is easy to obtain' and *Poètomu*[8] *estestvennee vybrat'* 'therefore it is more natural to choose', but no mention is made of such cases as (*Ja dumaju,*) *čto pravil'no vybrat'* (*pri takix uslovijax ešče trudnee, čem . . .*) 'I think that to choose correctly under such conditions is even harder, than . . .' or (*Nel'zja*) *tak pospešno sudit'* 'One mustn't judge so quickly', both of which fit the environmental requirements perfectly, but nonetheless contain obvious adverbs instead of predicate adjectives (group 3 contains close to 25% of the authors' predicate complements).

The situation cannot be said to improve in the following groups. Group I.4, "formula or noun or *čto/èto/to*+*-o/-e/-ee*+preposition or pronoun" is illustrated by the three phrases *pole perpendikuljarno k* 'the field is perpendicular to', *èto zakonno dlja* 'this is legitimate for', and , *to niže ee* ', now it is lower than it'. However, in the three identical or very similar phrases (*esli ustroim*) *pole perpendikuljarno k* 'if we set up a field perpendicular to', (*my budem delat'*) *èto zakonno vo vsex slučajax* 'we shall do that legally in all cases', (*dvigalos' to vyše*), *to niže ee* 'moved now higher, now lower than it', the ambiguous form, if not always a pure adverb, is certainly never a predicate complement. Similarly in group 5, "formula or noun+*-o/-e/-ee*+noun," we are offered two illustrations *Javlenie analogično èffektu* '(the) phenomenon is analogous to the effect' and *9 sil'nee ènergii* '9 is stronger than the energy', but what of the formally identical (*esli opisat' èto*) *javlenie analogično opisaniju* (*Ivanova*) 'if we describe this phenomenon in a manner analogous to the description by Ivanov' and (*ènergija A dejstvuet na*) *B sil'nee ènergii* (*V*) 'the energy of A affects B more strongly than does that of V', where the same forms *analogično, sil'nee* in the same immediate environment are not predicate complements, but adverbs? One of the illustrations of group 7, "nonverb, nor ACM+*-o/-e*+formula or colon" is *X niže X* (where *X* represents any

[7] Whether or not one counts the comma in determining environment is immaterial, since both *temperatura* and , are non-verbs. Apposition, which combines features of attribution and predication, is an important and interesting problem of Russian syntax. The article under discussion does not mention it.

[8] We have taken the liberty of standardizing the transliteration somewhat.

formula) '*X* is lower than *X*', but in the case of (*podnjat'*) *X vyše X* or (*spustit'*) *X niže X* 'raise *X* higher than *X*', 'lower *X* below *X*', the comparative is certainly not a predicate complement.[9] Group 8, "formula or noun+*-o*+period, comma, or bracket" is illustrated by *X postojanno* . '*X* is constant.' and *rezonansa izvestno* , 'of the resonance is known,' , but we find an adverb in the formally identical (*on rešal*) *zadaču ežednevno* . 'he worked on the problem every day.' and in the phrase (*pri takom*) *rezonanse izvestno, čto* (*peredača vosprinimaetsja lučše*) 'with such resonance it is known that the program is received better' the same *izvestno* plays an entirely different syntactic role.[10] Group 9, "nonverb+*-o*+, *čto* or , *čem*" contains adjectives in the examples *Xarakterno, čto* 'It is characteristic, that' (this is hardly a predicate complement, however) and *bolee zatrudnitel'no, čem* 'it is more difficult, than', but in the formally identical (*glaza reagirujut*) *bolee xarakterno, čem* (*uši*) 'the eyes react more typically than the ears', the *-o* form is an adverb. Group 12, "*kak*+*-o*+infinitive or nonverb" is illustrated by *Kak legko pokazat'* 'As it is easy to show' and *kak izvestno* , 'as it is known,'; one wonders whether there has been developed the computer program necessary to distinguish one *kak* from another and to produce the proper adverbial translation for such cases as *Kak legko èti* (*fakty možno demonstrirovat'!*) 'How easily these facts can be demonstrated!'.

The second major group, "Adverbial Modifiers," calls for somewhat less comment, since most of the groups therein are based on the undoubtedly correct assumption that an ambiguous form in *-o*, *-e*, or *-ee* occurring in the immediate vicinity of an adjective or of a verb is likely to be an adverb, not a short form neuter adjective. It may be useful, however, to distinguish between statistical probabilities and structural laws, and to note that even in this group one finds a number of cases for which there are formally identical phrases which contain not adverbs, but adjectives. For example:

Group II.1, "verb other than infinitive+*-o*/*-e*/*-ee*+irrelevant," e.g. *ocenivalos' vizual'no po* 'was visually determined by', ignores all combinations with forms of *byt'* 'to be' or of such semi-copulative verbs as *kazat'sja* 'seem', e.g. (*on*) *kazalsja niže, čem* (*Ivan*) 'he seemed shorter than John'. Group 2, "period or comma (may be followed by *a* or *kak i*)+*-o*/*-e*/*-ee*+

[9] In many such cases the ambiguous form seems to be part adjective, part adverb, in English as well as Russian. The example *raspoložennym perpendikuljarno k* 'placed perpendicularly to' in II.4 could in this writer's speech have been equally well rendered 'placed perpendicular to'.

[10] This role is defined by D. N. Ušakov, *Tolkovyj slovar' russkogo jazyka*, I, Moskva, 1935, col. 1145, as "in the meaning of a predicate."

comma not followed by *čto* or *čem*," is illustrated by three examples, for two of which one finds environmentally identical cases where the word in question functions as an adjective: in the original we have , *estestvenno* , ', naturally,' and , *a vozmožno, i* ', and possibly, also'; cf. for the first (*èto razvitie bylo, kak nam*) *dumaetsja, vpolne*[11] *estestvenno* , 'this development was, it seems to us, completely natural,' and for the second (*Takoe sledstvie konečno ne objazatel'no*) , *a tol'ko vozmožno, i* 'Such a consequence is of course not obligatory, but merely possible, and'. Group 5, "*esli* or auxiliary verbs or words[12] +*-o*+infinitive," is illustrated by *Esli otdel'no najti* 'If we find separately', *budet slabo zaviset'* 'will slightly depend', and *dolžno sil'no zaviset'* 'must strongly depend', but ignores cases with subject infinitive, e.g. *esli trudno najti* 'if it is hard to find' or *budet vospreščeno kurit'* 'smoking will be forbidden'. One is not surprised to find in group 6 that most *-o* forms are adverbs in the context "irrelevant +*-o*+ACM," e.g. *v sravnitel'no redkix* 'in comparatively rare', but one wonders how this group can occur in the same set of "preliminary rules" as the two groups I.10 "not verb+*-o*+*ètoj/ètomu/ètim*" and I.11 "*kotoroe*+*-o*+ACM or noun," since *ètoj* etc. are adjectives as well as pronouns, and since items such as *kotoroe* or "not verb" must either be relevant or not be relevant. Further, this group appears to ignore phrases with adjectives which govern oblique cases, e.g. (*pervoe*) *čislo proporcional'no vtoromu* 'the first figure is proportionate to the second', *uravnenie podobno vyšeypomjanutomu* 'the equation is similar to that described above', etc. Group 7, "dash+*-o*+ irrelevant" is illustrated by *—približenno* . '—approximately.', but fails to consider cases where the dash marks not hesitation, but the zero copula, e.g. (*takoe predpoloženie*)*—poverxnostno.* 'such a conjecture is superficial.'.

Major group III gives rules for determining the syntactic function of each of a series of two ambiguous forms. One cannot quarrel with the supposition that the first unit of such a sequence is probably an adverb, nor is it unreasonable to assume that when the sequence itself occurs in the immediate vicinity of a verb or adjective, the second unit therein is probably an adverb, whereas if there is no verb or adjective nearby, this second unit is likely to be an adjective. However, even here one must be cautious in speaking of "rules." Group III.1, "not verb or ACM+*-o/-e/-ee*+not verb or ACM," is illustrated by *metallax krajne zatrudnitel'no iz-za* 'in metals is extremely difficult because of' (this same example, without *iz-za*, occurs in I.1; cf. p. 184 above) and *sorta dostatočno malo.* 'sort is sufficiently small.'; compare with the second of these examples the environmentally identical

[11] Non- *-o/-e* adverbs are not counted in determining environment (p. 412).

[12] These are defined (p. 410, footnote) as including "words like *možno, nel'zja, možet byt', dolžny, neobxodimo*, as well as *vozmožnost', neobxodimost'*, etc."

(*Pojavljajutsja javlenija takogo*) *sorta dovol'no malo*. 'Manifestations of this sort appear rather rarely', where *malo* is not an adjective but an adverb, or (*Novogo on skazal v svoej*) *lekcii dovol'no malo*. 'He said rather little in his lecture that was new.', where the same word is a substantive.

The above examples, while not the only ones which might be adduced, should suffice to show that the so-called "preliminary rules" are really not rules at all, in the sense of being structural laws of Russian syntax. Rather, they seem (to this writer, at least) to be little more than elaborately formalized restatements of a number of translations which happen to be correct in a particular sample of Russian prose. If this is the case, then it is hardly surprising that the set of condensed "final rules" based upon them "results in correct translation 98.5% of the time" (p. 412); the circularity of this procedure should be obvious.

At the end of the article under discussion, after a list of words which occur in one function only,[13] there are given three additional rules, "constructed on the basis of regularities observed," which account for the remaining 1.5% of cases. As no examples are given of the first two of these, we shall glance only at the third, which reads, "If the word *malo* is followed by an adjective and the word *mesta*, translate it: 'there is little . . . space' (1 case)" (p. 414). One would like to inquire what sort of regularities can be observed in a single instance such as this, and also just where one draws the line between a rule (which, one assumes, should reflect some structural fact) and a simple translation (which may or may not reflect anything more than a simple semantic equivalence).

The three additional rules just mentioned are followed by what seems to the present observer an entirely accurate prediction, and one which penetrates to the center of this whole matter: "Thus, there remain no exceptions to our rules among 686 cases in the 31,000 word sample examined so far. *It is anticipated that the inclusion of the last three rules or the expansion of the entire set of rules may be required when larger samples of text will be analyzed*" (p. 414; italics supplied). As long as computers are taught to translate on the basis of "rules" which originate not in the real syntactic structure of Russian, but only in elaborate formalizations of a number of translations already arrived at by non-mechanical means, the translation of larger and larger samples of text can only give rise to larger

[13] In the case of some of these words a larger sample is sure to bring out new functions; e.g., the word *otnositel'no* 'relative(ly)' is listed under "Always prepositions," but one has only to look in the dictionary for examples of the same word functioning both as adverb (*Opyt prošel otnositel'no udačno* 'The experiment went off relatively successfully', Ušakov, *op. cit.*, II, col. 952) and as predicative adjective (*Ponjatie o blage otnositel'no* 'The conception of good is relative', *ibid.*, col. 953).

and larger sets of "rules," as the original "rules" are amended and expanded to cover all new cases. The eventual result of this technique would seem to be a dictionary of word-sequences, rather than of words alone. Given the capacity of modern computer memories, it is entirely possible that such a technique will eventually produce accurate translations in a very high percentage of cases. It is already certain, however, that as the stock of "rules" increases, the linguistic interest thereof will decrease, until we are ultimately provided with an enormous number of "rules" of no real interest whatsoever.

Now, it is none of the linguist's affair what an engineer does to enable a machine to produce translations, and he should be happy (in a somewhat disinterested fashion) were all such efforts to be crowned with success. The point here is that even if such a procedure is successful, it will leave the linguist exactly where he is right now, since the solution is itself extra-linguistic (if by "linguistic" we mean pertaining to the analysis of language as a system or set of systems). Fundamentally, what Koutsoudas and Humecky are doing is, although speaking in terms of analysis (morphemic, syntactic, etc.), actually begging the entire question of analysis by building up a set of phrase dictionaries. While it is possible that this solution will eventually provide all the accurate translations anyone could desire, the point is that this is all not linguistics, or even applied linguistics (whatever that is), but simply non-linguistics, and one questions the advisability of dressing it up in linguistic terminology.

To put it another way, one is disturbed by what seems the failure of many technicians engaged in machine translation projects to distinguish between the devices which happen to serve their limited purposes and the sort of reasonable approximation to truthful statements about language that professional linguists are (perhaps in a bumbling and certainly in an unmechanized way) trying to make. The two procedures are of course not necessarily mutually exclusive, and it may be that the search for handy technical devices will uncover some new structural laws along the way (although one sees little sign of this happening so far), but there is little indication that Koutsoudas and his co-workers realize just how great this difference is. To return to the article for an example: if it makes their machine work better, there is no reason why Koutsoudas and Humecky should not consider Russian *zakonno* to contain the suffix *-nno*; they are not, however, doing anything that could remotely be considered morphemic analysis.

There can be no objection to the use of contexts (linear or other) as such. It is clear that one weakness of Koutsoudas and Humecky's program lies not in its use of linear context but in its limitation of context to one

unit on either side of the ambiguous form. Yet one must not underestimate the seriousness of this weakness. Who would propose an analysis of homophonic morphemes based only on the preceding and following morphemes (e.g., do we have /-z/ 'singular' or /-z/ 'plural' in, say, the sequence *run-s-in* which can occur in both *John runs in the house* and *Mary has runs in her stockings*? Surely it is equally unpromising to analyze an ambiguous word purely in terms of the word or punctuation mark on either side of it. However, let us assume (for the sake of pursuing the theoretical point) that we can operate with a context consisting of the entire sentence in which a given form occurs, which for the present is about the maximum a machine could cover. What can we expect then?

First, we can probably resolve Koutsoudas and Humecky's particular problem without much difficulty (except perhaps for a few cases of sentence-level homonymity). It should not be hard to draw up a set of rules to the effect, say, that a form X occurring in the sequence of forms ABCXDE can have only the function $f^{\times}1$, whereas the same form occurring in the sequence LMNXOP can have only the function $f^{\times}2$. However, it must be emphasized that in doing this we are not analyzing the context at all, but are simply formalizing our own ability to understand these sequences (an ability which certainly takes into account many factors other than simple linear sets). We would in this case be doing (in a more thorough way) just what Koutsoudas and Humecky do in their article, namely making a direct jump from the environment in which a form occurs to the fact that we know what this form means, by-passing completely the problem of how we know what it means. This may be quite satisfactory for purposes of machine translation, but for a linguist the entire interest of course lies just in this "how."

As a matter of fact, as soon as we work on the level of complete sentences, the whole problem of these ambiguities really becomes more fiction than fact, since the ambiguity itself obtains only in the individual form, and seldom if ever in the form as one of a particular set of forms. To use Koutsoudas and Humecky's example: we can say that Russian *postojanno* is ambiguous (at least from the translator's point of view, 'constant' or 'constantly') when taken by itself, but there is nothing ambiguous about it in either of the sequences *Količestvo postojanno*. ('The quantity is constant') or *Količestvo menjaetsja postojanno*. ('The quantity varies constantly'). The same situation obtains in a more obvious way on the morpheme level, where we can say that /-a/ is in and by itself ambiguous, but not when one of a number of given morpheme sequences (/kn'íg-a/ nom. sing. fem. 'book', /stal-á/ gen. sing. masc. 'table', /pal'-á/ nom.-acc. pl. neut. 'fields', etc.). In other words, it may be the investigator himself

who creates his own difficulties as he sets up various descriptive categories (phonemes, morphemes, etc.) in the course of trying to extract the various systems of *langue* from the actual cases of *parole*.

To pass to more general considerations: although one hesitates to claim categorically that it would be impossible to get at the real syntactic structure of Russian through methods of linear context (such a statement would be premature, since—to our knowledge, at least—no one has even attempted such an analysis), one suspects rather strongly that such is indeed the case. What is the most that might be obtained from such an analysis? It seems that, by analyzing an enormous number of examples, and by asking native informants to play variations on the occurring themes, we might end up with a description of sequential possibilities of the type: "After # (juncture marker) any of the forms A, B, C . . . is possible; after the sequence # B only the forms X, Y are possible, etc., etc."; we could then make statements about our ability to predict the remaining portion of a sentence at any given point in the course of its production, and these statements might be quite interesting, particularly in conjunction with the analysis of ellipsis. It might also be possible to develop some sort of calculus working out from the interior of a sequence, although this probably would not produce as much of interest. It does seem, however, that this sort of analysis in the best case can never be more than a Markov-process description, and Chomsky[14] among others has demonstrated rather conclusively that language structure is a good deal more complicated than this sort of simple linear process. In other words, while we may indeed be able to describe permissible sequential combinations, the permissibility thereof depends on factors describable only on other levels, namely immediate constituent and transformation analysis.

What Koutsoudas and Humecky have done is to print an elaborate classification (under the name of "rules") of a mass of contextual information the bulk of which is demonstrably irrelevant to the problem they are trying to solve. The goal of this discussion (admittedly a negative one) has been to show this irrelevance, and to try to point out some of the differences between technical expediencies and linguistic analysis.

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[14] Noam Chomsky, *Syntactic Structures*, The Hague, 1957.

SEMANTIC SPACE REVISITED

A Reply to Uriel Weinreich's Review of *The Measurement of Meaning**

CHARLES E. OSGOOD

Psycholinguistics is a relatively new discipline developing along the border between linguistics and psychology. Both Uriel Weinreich and I are sympathetic to this development, and it is with the view of maintaining harmony along the border that I have written this addendum to his discussion of *The Measurement of Meaning*. Provincialism on either side of the border will hinder progress in interdisciplinary relations.

Weinreich's review is framed on the assumption that the semantic differential was developed as a technique for objectifying linguistic lexicography. He gives a convincing demonstration that the instrument is inadequate for this purpose, and then the authors are roundly criticized for their presumptiveness. But nowhere in the book is such a claim made by Osgood, Suci, and Tannenbaum; in fact, the term "lexicography" never appears on its pages. In the very last chapter, in a single paragraph, and in the context of possible applications of the technique, it is true that we mention the possibility of building "a functional dictionary of connotative meanings—a quantized Thesaurus," but this is a far cry from linguistic lexicography and even this development, as the authors clearly state, would depend upon considerable refinement of the measuring procedures.

The main point is this: The semantic differential was not designed as a linguistic tool but as a psychological one—to assess certain symbolic processes assumed to occur in people when signs are received and produced. It was not designed to classify or evaluate the innumerable correlations between linguistic signs and their referents—which really is not a psychological problem. Weinreich's attempt to make lexicographic distinctions with a version of "Twenty Questions" makes it clear that no standardized and limited set of dimensions could serve this purpose. But we have good evidence that the dimensionality of the *psychological* semantic space—the ways in which implicit, representational reactions can vary—is not so diverse and complex. The repeated appearance of the same general factors in replicated factor analyses (not only two, as Weinreich's review seems to

* "Travels Through Semantic Space," *Word* XIV (1958), 346–366, a discussion of *The Measurement of Meaning*, by Charles E. Osgood, George J. Suci, and Percy H. Tannenbaum, Urbana, Illinois, 1957.

imply, but nearer twenty), involving such varied stimuli as linguistic signs, facial expressions and even sonar signals and such varied subject populations as college sophomores, hospitalized schizophrenics, Japanese mono- and bi-linguals, and Navaho Indians, certainly suggests that this aspect, at least, of 'meaning' is capable of measurement.

Since that ugly word, 'meaning', has crept into the discussion, let's come fully to grips with the points raised by Weinreich on our usage of the term. He takes us severely to task for claiming that we are measuring 'meaning' when it is really the 'affect' of words, he thinks, that the semantic differential taps. Had we read even an elementary presentation of semantics, we would have been aware of the conventional statements of the problem of meaning and, at the very least, would have avoided such a crude dichotomy as 'denotative' vs. 'connotative' meaning. And to cap the point, Weinreich says in exasperated good humor, "But by the authors' own statement, these 'connotations' have (literally!) nothing to do with the referential capabilities or functions of signs."

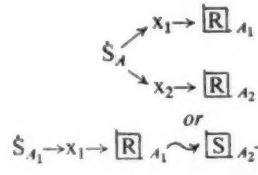
Now, without claiming to be as sophisticated as I probably should be with respect to philosophical and linguistic semantics, I would nevertheless say that there is nothing more confused and confusing than the literature I *have* read bearing on the usage of "connotative meaning." Furthermore, there are several "traditions" in the technical usage of the term 'meaning', one of which, represented by Ogden and Richards' "thought or reference" and Morris' "interpretant," refers to a representational state or process occurring in sign-using organisms when signs are received or produced. Also this is one of the standard usages in the lay language, e.g., when I ask someone to "Tell me what this inkblot *means* to you," he certainly doesn't assume that I want him to tell me what it refers to. In any case, there is nothing sacred about a "conventional" technical usage, particularly when it obfuscates the solution of a theoretical problem—as I think is the case here. Finally, we went to considerable pains (pp. 318–325) to specify how we were using the terms 'denotative' and 'connotative' and to indicate (pp. 2–10) what very limited *meaning* of 'meaning' the semantic differential is assumed to measure. But since our presentation was evidently unclear, and since I feel this is a critical issue, I will repeat the analysis here with some elaboration.

The ***denotative meaning*** of a linguistic sign I define as a conventional, habitual correlation between: (1) with reference to the speaker, a non-linguistic stimulus pattern, $\dot{S}$, and a linguistic reaction, $\boxed{R}$; or, (2) with reference to the hearer, a linguistic stimulus pattern, $\boxed{S}$, and a non-linguistic stimulus pattern, $\dot{S}$ (or a response, R, appropriate to this non-linguistic stimulus pattern). I use the symbols $\boxed{S}$ and $\boxed{R}$ to refer to

linguistic signs, as received or produced respectively, the symbol *S* for the thing signified (significate or referent), and the symbol *R* for a non-linguistic response. The *connotative meaning* of a linguistic sign I define as that habitual symbolic process, *x*, which occurs in a sign-user when: (1) a linguistic sign is produced (with reference to speaker); or (2) a linguistic sign is received (with reference to hearer). It is such symbolic, representational processes (*x*'s) that are presumably indexed by the semantic differential. The conditions for learning denotative meanings have been well described by Skinner in his *Verbal Behavior* (1957), and I have tried to describe the conditions for learning connotative meanings in my *Method and Theory in Experimental Psychology* (1953) and elsewhere.

Denotative agreement between two users of the same language exists when they display the same external correlations between linguistic and non-linguistic behaviors. *The crucial thing to note*, as shown by comparison of diagrams A and B below, *is that connotative agreement is not necessary for denotative agreement to occur—indeed, is entirely irrelevant to it*. In other words, as strange as it may seem on first thought, a common, shared psychological state or process between two communicators is not the *sine qua non* of denotative agreement between them.

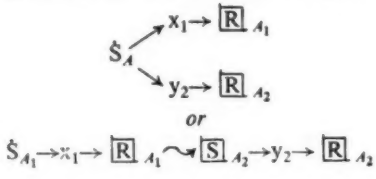
A. *Denotative Agreement plus Connotative Agreement.*


$\dot{S}_A \rightarrow x_1 \rightarrow [R]_{A_1}$
$\dot{S}_A \rightarrow x_2 \rightarrow [R]_{A_2}$
or
$\dot{S}_{A_1} \rightarrow x_1 \rightarrow [R]_{A_1} \rightsquigarrow [S]_{A_2} \rightarrow x_2 \rightarrow R_{A_2}$

EXAMPLE 1. When shown an APPLE object ($\dot{S}_A$) and asked to name it, both person 1 and person 2 say *apple* (R_A); when asked to say what they "think" about apples, or to fill out a form of the semantic differential, they give similar reactions, *x* in person 1 and *x* in person 2 (e.g., *sweet*, *tasty*, etc.), but this is irrelevant to the question of denotative agreement.

EXAMPLE 2. Speaker: *Bring me an apple!* Hearer brings this object, and the speaker says, *Thanks*. They both eat it with gusto.

B. *Denotative Agreement without Connotative Agreement.*

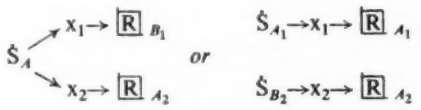

$\dot{S}_A \rightarrow x_1 \rightarrow [R]_{A_1}$
$\dot{S}_A \rightarrow y_2 \rightarrow [R]_{A_2}$
or
$\dot{S}_{A_1} \rightarrow x_1 \rightarrow [R]_{A_1} \rightsquigarrow [S]_{A_2} \rightarrow y_2 \rightarrow [R]_{A_2}$

EXAMPLE 1. Imagine a color-blind son, allergic to apples, and a normal father. Although apples don't look the same and certainly don't have the same gratifying consequences to the son as to the father (e.g., *x* in person 1, *y* in person 2), both will learn the same rules of usage—saying *apple* when APPLE is pointed to, bringing APPLE when requested with verbal signs, and so on. A machine could be built to do the same thing in a limited semantic area.

EXAMPLE 2. Person 1: *The late Senator McCarthy was an honorable man.* Person 2: *I know what you mean, but I disagree with you completely.* Here we assume that both people agree on the denotative meanings of *McCarthy* and *honorable*—testable by constructing such test frames as "McCarthy was Senator from Wisconsin during the early 1950's" and "You can believe the word of an honorable man." The reciprocally opposed profiles on the semantic differential for the concept THE LATE SENATOR MCCARTHY, but not for HONORABLE, for these two people would provide evidence for the disagreement in connotation. This, incidentally, is the most frequent application of the semantic differential—to index differences between people (patients vs. normals, Democrats vs. Republicans, etc.) in their connotations of signs on which they have denotative agreement.

Connotative agreement between two users of the same language exists when they make the same symbolic reactions, either to the same or to different stimulus patterns (which may be either signs or significates). As will be seen below, in this case *denotative* agreement or disagreement is irrelevant. If I am right, and we do have two independently variable conditions determining agreement in "meaning," then it is not only legitimate, but essential, that we discriminate with two terms—and I have chosen "denotative" and "connotative" as being the traditional terms closest to the distinction I wish to make.

C. *Connotative Agreement with Denotative Disagreement—Same Stimuli or Responses.*

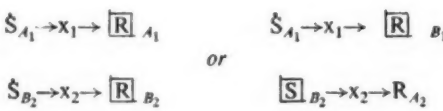


EXAMPLE 1. Child: *I like this cookie, Mommy.* Mother: *I like it, too, Johnny, but it's an apple, not a cookie.* Note that here we have a microcosmic bit of the learning process where connotative agreement aids in the development of denotative agreement in the usage of signs.

EXAMPLE 2. As is well known to students of international politics, both Soviet and American public speakers are prone to say *Democracy is good,*

etc. But despite connotative agreement (x in 1 and x in 2), the *referents* of *democracy* ($\hat{S}_A$ in 1 and $\hat{S}_B$ in 2) are quite different in the two cases, hence there is denotative disagreement.

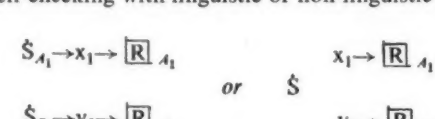
D. *Connotative Agreement with Denotative Disagreement—Different Stimuli or Responses.*


$\hat{S}_{A_1} \rightarrow x_1 \rightarrow [R]_{A_1}$
$\hat{S}_{A_1} \rightarrow x_1 \rightarrow [R]_{B_1}$
or
$\hat{S}_{B_2} \rightarrow x_2 \rightarrow [R]_{B_2}$
$[S]_{B_2} \rightarrow x_2 \rightarrow R_{A_2}$

EXAMPLE 1. The left-hand diagram illustrates in learning theory symbols the familiar phrase, "The gourmet approaches his food as a lover approaches his mistress." Both gourmet (person 1) and lover (person 2) have similar connotative states (x in both), but both the significates (food and mistress) and the linguistic labels correlated with them (*lamb chop* and *sweetheart*) are different—although under extremely intense connotative agreement, speakers may confuse the denotative correlations!

EXAMPLE 2. The phenomena of *synesthesia*, as communicated between two people, is a case in point. Person 1 says a loud tone seems *large* (R_B) to him. He presents a pair of tones, one loud and the other soft, to the second person and asks, "Which one of these seems to go best with the word *large*?" The second person indicates the loud one (R_A). We assume that the word *large* and the loud tone elicit similar connotative processes in both people (x in 1 and x in 2), and we can check this with the semantic differential.

If there is neither denotative nor connotative agreement in a communication situation, there is complete communication failure, which may or may not be self-checking with linguistic or non-linguistic means.



EXAMPLE 1. The same linguistic slogan, Right-to-Work Bills, refers to different things (freedom from union closed shop for person 1 and union-breaking technique to person 2) and also has entirely different evaluative connotation for the two people. Labor-management discussion using such a term would be difficult, to say the least.

EXAMPLE 2. Both teenager and her father espy a 1940 Ford, all fixed up in hot-rod fashion (S); *Cool*, says the daugher; *Junk*, says the father. Here the extra-linguistic knowledge that they are reacting to the same stimulus makes it possible for them to make inferences about each other's denotative and connotative systems.

EXAMPLE 3. Speaker 1: *Bring me a spider!* One is brought by person 2, whereupon the first person shrieks in surprise and fright. The first is a motherly old lady cooking dinner; the second is her young grandson, who is a budding naturalist. Here we have the homonym situation, which can only be clarified by the context. The differences between Uriel Weinreich and myself on the meaning of "meaning" may fit the same category!

The point of all this has been to justify the distinction we made in *The Measurement of Meaning* between "denotative" and "connotative" meaning. It may be true that this distinction comes out of a different tradition than that with which Uriel Weinreich is familiar, and in that case he is entirely justified in disapproving our usage of terms—indeed, he would be justified in saying that from his point of view our book was completely mistitled. But to imply, as he does throughout, that we have failed in our research efforts because we do not provide an index of *his* meaning of "meaning" (one that solves the problems of lexicographers), seems a little unfair to me.

However, Weinreich gives other reasons for being dubious about the success of our research venture. These concern the actual experimental and statistical methods we employed and therefore get into deep waters for the linguist. The reviewer makes a number of errors of interpretation which require correction. But in Weinreich's defense, let me hasten to say that I would certainly make errors of equivalent kind were I to attempt a critical review of a book on technical linguistics.

The first point of this sort concerns the adequacy of our sample, particularly of concepts to be judged. The reviewer refers to our sampling of only 20 concepts in each of two factorial studies as ". . . from a lexicographic point of view, almost laughably small. . . ." Letting pass the fact that we were not operating "from a lexicographic point of view," a linguist who often generates the grammar of a language from two or three informants certainly must realize that the size and nature of one's sample depends upon the variability in the dependent variable across the instances he is sampling. If (as the linguist assumes) there is very little variability across the speakers of a language in phonemics and grammar, then he is justified in using a small sample. Similarly, in the present case, if it can be shown that essentially the same factors emerge from *replicated* factor analyses using sets of different concepts (even if only 20 in each, or even

none at all as in one factor analysis Weinreich doesn't mention), then the same argument the linguist applies to himself applies here. However, for quite different reasons—namely, the evidence we have for concept-scale interaction—I am also acutely concerned with sampling concepts as broadly as possible; we made the best compromise we could think of between these purposes and the exigencies of subject effort and computer capacities, but entirely different approaches may be possible.

Yet Weinreich's main concern lies elsewhere. First he says, "The effectiveness of a factor analysis can be assessed from the proportion of the total variance of the data that is accounted for by the factors extracted." He then points out that in the Thesaurus factor analysis, most adequate from the lexicographical point of view, less than half of the total variance (about 40%, actually) is accounted for by 8 factors extracted. Then it is noted that for $\frac{2}{3}$ of the scales in this study less than 20% of total variance is taken out by the factors. Finally, he presents a table purporting to show what proportion of the total variance in judgments of 19 single concepts (based on separate factor analyses of the judgments of these concepts against the same 76 scales) is accounted for by the first five factors—the average value being only 27%.

In the first place, the proportion of total variance accounted for is not the only criterion of effectiveness of an analysis—in fact, this proportion is influenced by a number of subtle things. (A) It must be interpreted in relation to the *error variance*. Any psychological test, involving the judgments of subjects, is subject to some degree of unreliability; given the same items again, a subject will typically shift somewhat (e.g., where FATHER was judged 6 on the *relaxed—tense* scale, he now is judged 5 or 7), and the less relevant or clear the item the more shift can be expected. Even granting an average reliability of .80 to our scales (which is about the value we get on test-retest studies), this means that *the maximum possible reliable variance that could be extracted in an analysis would be* 64% (.80 squared) *of the total*. This means that our eight factors in the Thesaurus study accounted for approximately $\frac{2}{3}$ of all that could be extracted. (B) The proportion of total extractable variance actually taken out also depends upon the intercorrelations among the scales, and hence upon their selection. One factor *could* account for almost all of the reliable variance *if* we deliberately selected scales which were close synonyms of each other. This is what I think tended to happen in the earlier analyses, based on the scales most frequently given as qualifiers of nouns from the Kent-Rosanoff Association Test stimuli: nearly $\frac{2}{3}$ of the scales yielded were clearly evaluative in nature. Now in the Thesaurus study--for precisely this reason (as we stated in the book)—we deliberately *pruned out* close

replicates of scales, trying to get as large as possible a number of independent scales. Naturally this reduced the proportion of total variance extracted. This, however, does not deny the validity of Weinreich's general point—that there are presumably a large number of factors still unidentified. Yet I would still claim that their contribution to total variance in human semantic judgments must be relatively small and (for many purposes other than lexicography) unimportant.

Secondly, as far as the variance (h^2) of particular scales is concerned, this depends upon both the sampling of concepts and the sampling of other scales. If there are few if any concepts relevant to a particular scale, say *wet—dry*, then most judgments will be near the midpoint of the scale and correlations with other scales (which determines common or shared variance that can be extracted) will necessarily be low. If the sampling of other scales has failed to include a few of similar meaning, again the correlations will be low and the variance extracted on common factors infinitesimal. Ideally, of course, we should have infinitely large samples of both concepts and scales, but this is practically unfeasible. (Without ILLIAC, a high-speed digital computer, even the 76×20 Thesaurus factor analysis would have taken many months or even years to complete.) This means that many potential dimensions are lost in the effort to demonstrate that a few of the basic, dominant ones are stable and reproducible.

Finally, Weinreich's Table B and the discussion of it implies that five factors were given (in our Table 30) for each of the concepts whose total variances are represented. Actually our table gives the five dominant factors in the total analysis (all concepts combined) and then lists for each concept only those factors of the five which could be clearly identified in the single-concept factor analyses. So we find five factors listed for only four of the nineteen concepts, four factors for nine concepts, only three factors listed for five concepts, and only two factors for one concept. Dividing the variance extracted by the actual number of factors, we find an average per factor of 7%, which for five factors per concept would be 35% of the total variance. Keeping in mind that only *reliable* variance can be extracted by common factors, this means that we are accounting for an average of more than half of the variance that could be accounted for.

If, as is the way with factor analyses, the first factors extracted tend to be the largest ones and subsequent factors decrease in the percentage of total variance according to a negatively accelerated function, then we can envisage a long, long "tail" of minute, specific factors extending toward, but never quite reaching, the limits set by reliability. Solution of the lexicographer's problem may require both the "head" *and* the "tail" of this distribution of semantic factors, but there are many psychological problems

and psycholinguistic problems which can be attacked with just what we know about the "head"—the first three or four, most significant and repeatable factors.

I have written this reply to Weinreich's review of *The Measurement of Meaning* in part because I wanted to balance the scales a bit and in part because we represent two disciplines with different traditions and problems (yet which have been interacting fruitfully over the past few years). It may be that our research has no relevance to *linguistics per se*, but I would like to think that it is of some interest to *linguists* as students of language behavior more generally. Uriel Weinreich closes his review with an allusion to Columbus' voyage to what he thought was India but turned out to be a quite different place. I think we know where we're headed, all right —even if the name by which it is called changes with the map one uses— but we've barely set sail, our boat may be leaky, and already the seas are rough.

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A REJOINDER

URIEL WEINREICH

1. I do not feel that any grave unfairness was committed by evaluating *The Measurement of Meaning* from the lexicographic point of view. First—and this is to the authors' credit—there is sufficient lexicographic relevance in the theoretical parts of the book to merit a critical discussion of this aspect of the work. Secondly, my discussion was not at all "framed on the assumption that the semantic differential was developed as a technique for objectifying linguistic lexicography." I think that in the last two paragraphs of §1, I explained the very special nature of my approach to a book which has many other worthy purposes.

2. It would be idle to argue about terms; Osgood has now fully amplified what he means by 'connotation' and 'denotation'. The chief defect of his theory, however, is (to my mind) the absence of a level which is, in some ways, "intermediate" between his 'denotation' and 'connotation'. If the former deals with relations between signs and their referents, and the latter, with relations between signs and their users, there is missing the linguistically crucial domain of relations between signs and other signs

such as is expressed in a (non-ostensive) definition. Almost every semantic theorist to date has distinguished this "ability of signs to refer" from their actual referring; cf. Frege's 'Sinn'/'Bedeutung', Husserl's 'Bedeutung'/'Bezeichnung', 'innere Form'/'Bedeutung' in the Humboldtian tradition (especially Marty), Mill's 'connotation'/'denotation', Paul's 'Bedeutung'/'Benutzung', de Saussure's 'valeur'/'substance', Carnap's 'intension'/'extension', Hjelmslev's 'form'/'substance' (of content), Quine's 'meaning'/'reference', Morris' 'designation'/'denotation', etc. Osgood's 'denotation' corresponds roughly to the second member of each pair of concepts, but his 'connotation' (which I tried to rename 'affect' or 'emotive influence') does not pertain to this dichotomy at all. It is widely, even though not universally, agreed that it is the first member of each of the above pairs—the one that has no equivalent in Osgood's theory—that is of interest in the description of language.

3. In regard to the effectiveness of the statistical procedures, Osgood's reply serves to demonstrate the divergence in our views. He argues skillfully that his factor analysis is nearly as good as could be *expected* from its design. Be that as it may, I still feel that it is not as good as is *required*, and thus I remain skeptical about its value for semantic analysis as the linguist must practice it.

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Reviews

JOSEPH H. GREENBERG, *Essays in Linguistics*, vii+108 pp. Chicago: The University of Chicago Press, 1957.

Reviewed by NOAM CHOMSKY

This volume contains a group of rather loosely related papers touching on a wide range of linguistic topics. Chapters III–VI, dealing with methods for determining genetic relationship and subgrouping and the problems caused by convergent development, drift, borrowing, etc., are primarily review and summary, oriented towards anthropologists, but contain a number of observations that will interest the specialist as well. Chapter VII, after a discussion of uses of the words 'function' and 'structure' in social science, contains suggestions about possible quantitative studies and remarks about meaning and various uses of language. Roughly by 'structure' in this discussion is meant a system of units (e.g., a phonemic system) and by 'function', the manner in which these units are organized in "higher-level" structures (e.g., phonemes in the morphemic system, sentences in general social behavior).

Chapters I, II and VIII are more original contributions to linguistics as such. Chapter VIII is concerned with the interesting problem of determining linguistic universals, that is, features which specifically characterize natural language, among sign systems of a very general type, or which occur in language with a significantly high frequency. The particular feature considered is order of affixing. Greenberg points out (without actually presenting detailed evidence) that suffixing is much more common than prefixing in natural language, and tries to find a psychological explanation for this fact. Although the problem posed is significant, the particular explanations advanced are not, unfortunately, particularly convincing. One of the proposals (based on "vividness") is too vague to be very useful, and a second (based on material concerning similarity and dissimilarity of stimuli and responses) seems hardly relevant, since there is no sense in which, e.g., a stem can be considered a response to its prefix as stimulus (a point mentioned by Greenberg himself). The latter attempt at explanation also rests on a conception of a sentence as "a chaining of successive responses" (p. 90) which is sharply in conflict with what is known about

grammatical structure, as well as being open to fundamental objections on psychological grounds.[1] A third proposal is based on the observation that the class of stems is much larger than the class of affixes. Assuming, then, that the speaker will tend to select that order of elements which will "orient the hearer to the appropriate reactive behavior as soon as possible" by selecting the members of larger classes first,[2] it will follow that stems should, in general, precede affixes. However, the assumption is very dubious on empirical grounds. If it were true, one would expect to find the basic word order NNV predominating, auxiliaries following verbs (not, e.g., as in English), pronouns always occurring last in sentences, etc. Furthermore, the explanation again assumes the unacceptable chaining theory of sentence production. We are left with a suggestion based on the observation that "in regular conditioned sound changes, the conditioning factor is far more frequently a sound which follows than one which precedes." This would lead us to expect that prefixes would tend in time to lose independent status as morphemes through assimilation and merger. That is to say, prefixes would be unstable and hence suffixing would come to predominate. This proposal seems more promising.

The first essay and three appendices deal with general formal properties of sign systems and (as a special case) of language, again a very important and insufficiently studied topic. To obtain non-trivial results in this area is not easy, however. In order to show the applicability (in any serious sense) of mathematics to the study of grammatical structure it is not sufficient to demonstrate that certain formal properties of language enable it to be regarded as a particular case of some well-studied mathematical structure. There are many trivial ways to do this. Thus it is easy to construct an enumeration of all finite sequences of the letters of the alphabet (e.g., let any sequence precede each longer sequence; of two sequences of equal length, let the lexicographically earlier, in the obvious sense, precede). Since the enumeration includes all sentences, we can state the problem of grammatical investigation as that of specifying a certain function on the integers, namely, the function which gives the value one for

[1] Cf. K. S. Lashley, "The Problem of Serial Order in Behavior," *Hixon Symposium on Cerebral Mechanisms*, ed., Jeffress, New York, 1951.

[2] The assumption is that at each point in the sentence there are a certain number of remaining possibilities as to what the sentence will be, and that choice of an element out of a larger class reduces this number more severely than choice from a smaller set, thus giving more "information" to the listener as to which sentence is being selected. However, this interpretation is unacceptable as it stands. Since, by and large, there are, at a given point in a sentence, an infinite number of possible sequences which will complete this sentence, choice of a particular element does not reduce the *number* of remaining alternatives at all.

each argument n such that the n^{th} sequence in the enumeration is a sentence, and the value zero elsewhere. Thus grammar becomes part of a well-studied branch of mathematics. Though this might conceivably be the first step in a development of significance, such an "application" of mathematics to linguistics would obviously not yet be of the slightest interest. For significance, we must find some aspect of linguistic structure which can be studied abstractly in such a way as to yield conclusions which tell us something new about the empirical subject matter. For example, we might try to show that the functions associated (as above) with grammars of natural languages belong to some special class of functions with interesting and unexpected properties, and we might try to conclude from this something about deeper structural properties of language.

A strong argument can be made for the contention that abstract study of formal properties of grammars is, at this point in the development of linguistics, a very hopeful way to achieve new and deeper insight into language structure. Although Greenberg is one of the very few who have given serious thought to this question, I do not think that the work presented here yet leads to any real results. All that is shown is that several algebraic notions can be given an empirical interpretation in linguistic material. This in itself is no more interesting than the remark that a grammar can be thought of as a number-theoretic function. Several highly important problems are raised, but not developed sufficiently. In particular, Greenberg poses the problem of analyzing the structure of the rules needed to characterize the grammatical sentences from among all the sequences in some alphabet (5f.). This is the fundamental mathematical problem in the theory of grammar. But the discussion here sheds very little light on the question. Several of the classes of rules discussed are totally useless for the purposes of grammar (e.g., rules excluding sequences of certain lengths); the others are not developed to the point where their importance and implications can be perceived.

A large part of the chapter is devoted to the development of various notions of substitution class. Suppose that we regard a language L as a set of finite sequences of certain units (i.e., a set of sentences). Substitutability can now be defined in many different ways. Let us say that two units A and B are substitutable$_1$ if there are expressions X and Y such that XAY and XBY are sentences of L; substitutable$_2$ if whenever XAY is a sentence of L, then so is XBY, and whenever XBY is a sentence of L, then so is XAY (i.e., A and B are completely mutually substitutable). These are the simplest and basic notions.[3] On the basis of these, a variety of different

[3] They are discussed by R. Carnap in *The Logical Syntax of Language*, 1934 (New York, 1937), p. 169.

substitutability concepts can be constructed.[4] Greenberg mentions three of these possibilities. Given a context X—Y, the *contextual class* defined by X—Y is the set of units A such that XAY is a sentence of L. As Greenberg points out, contextual classes are not very useful. Secondly, suppose that $X_1, \ldots, X_n$ are expressions of the same length, each of which differs from that preceding it in the sequence just in the substitution of a single unit (e.g., if X_1 is abc, then X_2 may be dbc). In this case call X_1 and X_n *equivalent*. Select a maximal class of equivalent expressions. Then for each i, the units which occur in the i^{th} place in the expressions of this maximal class constitute what Greenberg calls an *extended class*.[5]

A third notion, that of *restricted class*, is not very clearly defined, but seems to come to this. Let X, Y be expressions such that XAY is a sentence of L, where A is a unit. We say that ZBW is *derived* from XAY if B is a unit (perhaps identical with A); Z is either X or is formed from X by addition or deletion of elements (but not both); W is either Y or is formed from Y similarly. Then C and D are substitutable$_3$ (with respect to the context X—Y) just in case whatever Z and W may be, ZCW is a sentence of L derived from XAY if and only if ZDW is a sentence of L derived from XAY. The restricted class determined by the context X—Y is then defined as the class of units which are mutually substitutable$_3$ with respect to X—Y.

The latter two notions appear to me somewhat peculiar. I see no particular linguistic motivation for defining substitutability in just these ways, nor do I see any obvious and interesting formal properties of these notions, though there might be some point in studying the relation between these notions and some of those developed in the papers cited in footnote 4.[6] Greenberg intends the notion *restricted class* to be a precise

[4] Several are developed in Y. Bar-Hillel, "On Syntactic Categories," *Journal of Symbolic Logic* XV (1950), 1–16. A notion based on substitutability$_1$ of a different kind, and, I believe, of potentially greater usefulness for linguistic analysis, is proposed by Z. S. Harris in "Discourse Analysis," *Language* XXVIII (1952), pp. 5f., and developed in further detail in Chomsky, "Systems of Syntactic Analysis," *Journal of Symbolic Logic* XVIII (1953), 242–256. Still other notions appear in Harris' *Methods in Structural Linguistics*, Chicago, 1951, chapter 15.

[5] The example of an extended class given on p. 10 contains an error in that the cited chain produces not q and s, but p and s in the second position, and the chain introducing q into this class is not (but can be) given.

[6] Thus it seems likely that under fairly reasonable conditions that might be imposed the notion of restricted class would be close to or identical with that of substitution class based on substitutability$_2$, and the notion of extended class would turn out to be a special case of the notion proposed by Harris in "Discourse Analysis."

reconstruction of the ordinarily used notion of substitution class. To illustrate its use he gives the example of the context "I see —" in which we can place *well, houses* and *rooms. Well* and *houses* belong to the same contextual class, but not the same restricted class, since the context can be expanded to "I see very —." *Rooms* and *houses*, on the other hand, are said to belong to the same restricted class. This is an error, however. If we expand the context to "I see bed—" or "I see that John — here," *rooms* but not *houses* can be inserted. In general, restricted classes are rarely found, and they do not seem to be the most useful classes for linguistic description. My own guess would be (and there is nothing in the literature to refute this) that the kinds of classes that linguists usually set up and find most useful cannot be developed by mere elaboration of direct substitution procedures.[7]

Chapter II, dealing with procedures for defining 'morpheme' and 'word', is the most substantial paper in this volume. It contains new ideas, and deserves close study. There is an important difference between this essay and the preceding one which should be carefully noted. In a sense, they belong to quite distinct fields of investigation. Chapter I is concerned with the general formal properties of natural language and related systems. It thus belongs to a domain that should be called "linguistic theory." Chapter II, however, is an essay in methodology; that is, it attempts to provide a procedure for arriving at the structure of an arbitrary unknown language. Both essays are general and, in a sense, abstract, in that they do not deal specifically with a particular language. But there may be a vast difference between, on the one hand, a theory of linguistic structure that specifies abstractly the general properties of natural languages and grammars, and, on the other, a mechanical method (if one exists) for determining the specific way in which a given language realizes this general structure. Methodology has been a primary concern of theoretically oriented linguists; linguistic theory, in the proper sense of the word, has

[7] Greenberg also asserts that inflections and roots can be formally distinguished in terms of restricted classes (p. 12). His proposal is to define a morpheme as inflectional if replacement by a different morpheme will change the restricted class of the construction (i.e., word) to which it belongs. This will not do, however. One difficulty is that restricted classes are defined relative to a fixed context, and there are contexts in which different inflectional elements can appear without changing the substitution class of the word to which they belong. More seriously, there are a great many morphemes which are not inflections but which constitute, in many of their contexts, a restricted class in themselves (i.e., a class with one member), e.g., *be, have, become, in, all, very*, etc., and even such words as *green* (because of *greenhorn*), *black* (because of *blacken, bootblack*), and so on. Again, my guess would be that it is not possible to develop a precise reconstruction of the root–inflection distinction on such a narrow basis.

not. Hence although chapter I is a good deal less substantial than chapter II, it represents a more radical departure, in an important and neglected direction, from traditional concerns.

In developing his procedure for morphemic analysis, Greenberg distinguishes carefully between the problem of deciding *that* a certain sequence of phonemes must be segmented, and the problem of deciding *where* it is to be segmented. Correspondingly, two methods of analysis are outlined. Given an utterance, we first apply method A to determine whether it must be segmented; then we apply method B to determine points of segmentation. We then reapply the two methods to the segments produced by the first application of methods A and B, etc. Now no matter how these methods are formulated, it is evident that the results of analysis of one utterance cannot be freely applied to the analysis of a second utterance because of the possibility of homonymity. For example, if in one utterance these methods lead to the analysis of /tæks/ into *tack* and *s*, we cannot merely carry over this analysis to another utterance containing *attacks*, *tax*, etc. Similarly, if our methods provide us with the analysis *fly*+*ing*+*plane*+*s* for the phrase occurring in the context "— is dangerous," we cannot safely conclude that the same analysis is appropriate for the same sequence of phonemes occurring in "flying planes are dangerous."[8] It appears, then, that even if methods A and B are simple and effective, the procedure based on them will be of incredible complexity if only because of the number of applications of this procedure that will be required in even a corpus of moderate size (we will see furthermore that the methods are neither simple nor effective). It becomes quite relevant, therefore, to raise the question of just what is the purpose of these methodological studies. We return to this below, after considering the specific methods developed.

Greenberg proposes what he calls the method of "squares" as a procedure for determining whether a sequence of phonemes is to be segmented (method A). Suppose that we have four expressions X, Y, Z, W. Then there

[8] The fact that in this case the conclusion would be correct is irrelevant. If we were to allow the inference in this case, we would have to allow it for the former example, too. A separate essay could be written on the difficulties posed by examples of this sort for any approach to morphemic analysis which, like that of Greenberg's, claims to be based on meaning but not syntactic structure, since here the meaning of the phrase is not determined by the meaning of its components, and if meanings are not somehow held constant throughout the successive applications of procedures of morphemic analysis, absurdities such as the analysis of *tax* into /tæk/+/s/ will result. An approach to morphemic analysis that is willing to consider syntactic considerations as relevant will, in many cases at least, not face this difficulty.

are three kinds of situation in which we will regard these sequences as segmentable.[9]

(1) X=AC, Y=BC, Z=AD, W=BD, and there is "corresponding variation in meaning."

(2) Z and W fit into a square of type 1; the meaning relation of X to Y is the same as that of Z to W; there is a context in which X and Z, but not Y and W, can occur, and a context in which Y and W, but not X and Z, can occur.

(3) Same as 1, but without the requirement of corresponding variation in meaning.[10]

Basically, (2) differs from (1) in that the requirement of phonological similarity is dropped, and (3) differs from (1) in that the semantic requirement is dropped.

Case (1) is the most stringent of these three conditions. It is necessary to impose it, Greenberg argues, to eliminate such squares as *trail*, *sail*, *trip*, *sip*. To my knowledge, Greenberg is the first of those methodologists who claim that morphological analysis can be based on meaning to propose a method for avoiding this difficulty which, in one form or another, has often been raised as an objection to the introduction of semantic criteria in morphological analysis. It is not obvious, however, that the semantic condition in case (1) succeeds in eliminating this square. Thus we may assign to *tr* the meaning 'travel' (as in *travel*, *truck*, *train*, *trolley*, etc.) and to *s* the meaning 'water' (as in *sea*, *surf*, *spill*, etc.). The suffixes *ail* and *ip* indicate 'activity involving . . .'; *ail* is a verbalizer, while *ip* is a

[9] A fourth condition is introduced to take care of the familiar problem posed by such words as *huckleberry*. Reliance on a semantic criterion in this case also raises special problems. Thus it is difficult to see why, as the semantically based criterion is formulated, the word *manager* should not be analyzed *man-ager*, *-ager* meaning 'in charge'. (The fact that a woman can be a manager is not too troublesome, since *woman* can be analyzed into the feminine prefix /wú/—plural /wí/—with /mæn/ undergoing an automatic reduction of vowel in unstressed position. *Man* and *woman* will thus fit into a square with *prince*, *princess* (see below) and we can assume that in *manager* the zero affix of masculinity assigned to *man* in the square is missing—that is, gender is "neutralized" in this case.)

[10] Greenberg's description of case (3) is misleading, since it seems to imply that case (3) differs from case (1) in that meaning cannot be assigned to the component morphs A, B, C and D in case (3). But this possibility of assigning meaning to the component morphs is not required in his description of case (1). From the examples for which case (3) is specifically designed (see below), it is clear that what is missing is lack of corresponding meaning variation in the square.

nominalizer.[11] Similar examples are easy to construct, with a little ingenuity: e.g., *walk*, *talk*, *wipe*, *type*; *morning*, *evening*, *moral*, *evil*; *flip*, *flop*, *drip*, *drop*; etc.[12] All such examples will obviously be absurd, since it is absurd to assign meanings to what are in fact parts of morphemes. But this is of course beside the point in this methodological discussion.

The paradigm examples of case (1) that Greenberg cites are *eating*, *walking*, *eats*, *walks* and *king*, *kingdom*, *duke*, *dukedom*. If we interpret the notion "corresponding variation in meaning" fairly loosely, the examples are appropriate. But a moderately sophisticated semantic analysis would, I believe, lead us to reject these squares. Thus *walking* but not *eating* is natural in such contexts as "a(n) — dog," "a(n) — apparition," though both *eats* and *walks* give natural (though not necessarily sensible) sentences when placed in the corresponding contexts "the dog (apparition) — slowly."[13] I presume that this indicates absence of corresponding variation of meaning in this square.[14] Similarly, the second example cited is dubious. The meaning of *kingdom*, but not *dukedom*, is such as to permit such phrases as "the animal kingdom." This should again lead one to conclude that the square is semantically deficient. In general, it appears that few squares will stand up to a stiff semantic test. Exact correspondence in meaning variation will be at least as hard to find and demonstrate as exact synonymity. In order to apply case (1) effectively, then, we must somehow make the semantic test loose enough to permit the instances cited in this

[11] That is, we are considering occurrences of *trail* and *sail* as verbs, and *trip* and *sip* as nouns. Recall that distinct occurrences must be analyzed separately. In this meaning assignment, we have actually gone beyond what is necessary, since "meanings" have been assigned to the component morphs, while all that is required is that corresponding variation in meaning in the full expressions be found. Of course, the meanings of the "compounds" *trail*, *sail*, *trip*, *sip* are not totally predictable directly from those of their component morphs, any more than the meaning of *nationality* is directly predictable from the meanings of its components. This failure of direct predictability is the rule, if we really take meaning seriously enough to describe it accurately.

[12] Thus *w* means 'non-verbal'; *t* means 'verbal'; *alk* refers to an activity not involving the hands; /ayp/, an activity involving the hands; *ning* indicates 'time of day' while *al* is an adjectival ending; *mor* indicates 'light' and /iyv/, 'dark' (the connotations of light and dark in the phrases "moral acts," "evil acts" are obvious and familiar). And so on in many other cases.

[13] This is in fact an instance of what appears to be a much more general syntactic regularity of English, involving a distinction between pure intransitive verbs (like *walk*) and mixed transitive-intransitives (like *eat*). Cf. Chomsky, "A Transformational Approach to Syntax," to be published in the Proceedings of the University of Texas Conference on Syntax, May, 1958 (§8), for further discussion.

[14] It is difficult to decide this clearly, because of the compounding of obscurities involved in such a notion as "corresponding variation in meaning."

paragraph but stringent enough to exclude those of the last paragraph. It seems to me that possibilities for doing this in any clear way are so remote as to indicate that the whole approach is misguided.[15]

The most striking difference between the examples to be included and those to be excluded by case (1) is a difference in the syntactic function of the component morphemes, i.e., a difference in the way the proposed analyses would fit into a set of very general statements about the construction of English sentences. This becomes even more obvious when we consider such examples as "John's refusal" or "what did he have done (to his teeth)?" where it is rather pointless to attempt to assign meaning directly to the component morphemes, though their syntactic function is clear. It seems to me that considerations of this sort are critical for morphemic analysis (whether they are sufficient is another question). In fact, Greenberg refers to these considerations as metacriteria for choice of methods of analysis.[16] But the attempts of Greenberg and others to construct mechanical procedures to meet these metacriteria show no indication of succeeding, as far as I can see. An alternative approach would be to concentrate on precise formulation of the metacriteria (i.e., to develop a measure of chaos and coherence) and to use this directly as a method of

[15] Greenberg suggests a translation technique to provide additional evidence about the appropriateness of a square. But basing morphemic analysis in part on the notion "preservation of sufficient correspondence of meaning variation under translation" stretches intuition well past the breaking point. Surely this notion is far more in need of clarification than the notion 'morpheme' itself. That is, at this point linguistic theory will be better off simply taking the term 'morpheme' as an unanalyzed primitive notion. Furthermore, I do not see how the very difficult question whether another language makes a semantic distinction exactly parallel to that of English *national—nationality*, *eats—eating* (as in "is eating"), etc., can be considered at all relevant to the analysis of these units (or is only "near preservation of . . ." required by this test?). Citation of such evidence would be no more relevant than reference to French translation to show that *hair* and *candy* are plural in "meaning," or reference to English paraphrase (as *garb*) to show that *clothes* is singular. For that matter, it is no more relevant than citation of a phonemic distinction in language A to justify the claim that the distinction exists in language B. Finally, there is surely no hope of solving such notoriously difficult problems as that of determining the correct semantic analysis of the English verbal system, in the absence of *any* information about English morphology. Yet it is just this that is required if this method is to be applied. Or, if we are to rely on bilingual dictionaries, as in Greenberg's example (p. 20), we might as well use the even simpler "method" of adopting the morphemic analysis given by a good dictionary directly.

[16] "The attempt to describe these Semitic forms without such segmentations would reduce Semitic grammar to virtual chaos." ". . . The justification . . . [for these procedures is] . . . their convenience—indeed their practical indispensability—for a coherent grammatical description of a language" (p. 22). "Moreover, it is generally believed that segmentation is required in such cases as *men* for grammatical purposes" (p. 21).

justifying grammars, however arrived at, thus giving up the hope, for the present at least, of finding simple and direct mechanical procedures for the discovery of the optimal grammar of a language. In terms of the distinction drawn above, this would mean, at the same time, a shift of interest from linguistic methodology to linguistic theory.

It does not appear that we can easily accept the assertion that the most stringent condition, namely case (1), "will always give valid and generally accepted analyses" (p. 20). When we turn to the "defective" cases (2) and (3), the situation is much worse. For example, case (2) will impose an unwanted morphemic segmentation on *bull* and *cow* (also *boy*, *girl*; *king*, *queen*; etc.) because of the squares *bull*, *cow*, *lion*, *lioness*; *king*, *queen*, *prince*, *princess*; etc. In fact, the final condition in the statement of case (2) was designed explicitly so as to exclude this possibility, but it fails because of such contexts as "the — hurt him(her)self."[17]

Case (3) lets the bars down completely. It permits, for example, the analyses cited in the discussion of case 1, without any necessity for seeking exotic meaning correspondences.

However, cases (2) and (3) are quite necessary, as Greenberg points out. Without case (2), it will not be possible to analyze *man—men*; and without case (3) it will be impossible to analyze Semitic verb forms or English *deceit*, *receipt*, etc. In fact, case (3) is needed much more generally than this, e.g., if we are to be able to analyze such forms as "look it up," "throw it out," etc., where the appropriate meaning correspondences cannot be found.

We have discussed only English examples. In languages with complex morphology such as Semitic, however, a great many new difficulties arise. E.g., consider the Hebrew square /huyilmad/, /huyavo/, /hitilmad/, /hitavo/ ('he will learn, he will come, she will learn, she will come'). There is only one segmentation compatible with this analysis, namely, into /huy/, /ilmad/, /hit/, and /avo/. But there are in fact word boundaries after /hu/ and /hi/ (not necessarily phonetically marked, however) and only there.[18]

[17] In a language such as Hebrew where squares of a similar character are easy to find and verbs and adjectives are marked for gender, the failure of this criterion is much more evident.

[18] The method of arrays (see below) will not remedy this, as it is here formulated. The correct morphological analysis of /yilmad/ is into a root /lmd/, a vowel pattern /aa/ (the first member of which is dropped by an automatic morphophonemic change), and the third person morpheme /y/. It is pointless to assign the /i/ to any morpheme at all; it is simply introduced by an automatic rule of great generality involving initial sequences CCC. (This is a good example of the general unjustifiability of the requirement of "morphemic accountability," i.e., the requirement that each phoneme must be assigned

We have so far discussed only method A, the method for determining whether a given sequence is segmentable. Greenberg also has some novel suggestions about a method (method B, above) for determining where the boundary lies. To demonstrate the necessity for method B in addition to method A, Greenberg cites examples of two different types. First, note that in the square *walking*, *talking*, *walks*, *talks*, the point of division is not uniquely determined. It might be after *w*, *t*, or after *walk*, *talk*. Secondly, consider the Arabic square *qatala*, *ḍaraba*, *qutilu*ʿ, *ḍuribu*ʿ, from which we would like to extract the morphemes *qtl*, *ḍrb*, *aaa*, *uiu*ʿ. But our method for doing this must not permit the corresponding analysis of English *beats* into *bs* and *eat* because of the square *beats*, *breaks*, *beating*, *breaking*.

To solve these problems, Greenberg proposes the method of arrays. An array is to consist of all the perfect squares[19] in which a form figures, and is to be constructed in accordance with the convention that forms whose communis precedes their propria are written in a row, and those whose propria precedes their communis are written in a column. Since arrays are based on squares, the difficulties noted above carry over to the new method.[20] Furthermore, new difficulties arise. The instructions for constructing an array are, as stated, mutually contradictory. It is impossible to introduce all perfect squares into an array that meets the convention, since in many squares communis and propria are interspersed, hence neither precedes the other. In the example of an array given in the text, several of the entries fail the convention (e.g., one row contains *shake*, *shook*, but the communis is /š—k/ and the propria /ey/, /u/). Furthermore, neither the Arabic nor the parallel English square given above as providing the motivation for developing the method of arrays can be introduced into an array for this reason. To remedy this is not as easy as it might seem. Thus the obvious suggestion would be to stipulate that the communis precedes the propria in case some part of the communis precedes all of the propria. But this revision will still make it impossible to introduce the

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to some morpheme.) Greenberg's rule 4, p. 24, will, incidentally, assign the /a/ to the morpheme /lmd/ (because of such parallel forms as /yigmor/), thus complicating the morphological description needlessly. In general, I do not see any possibility of arriving at an adequate analysis of a language with such a complex morphology as that of Hebrew by a procedure that does not consider as relevant the way in which particular morphemic analyses affect the complexity of the total grammatical description; on the other hand, if this is taken into account, many facts fall into place quite neatly.

[19] It is also extended to include defective squares.

[20] Thus we have in fact found the square *walk*, *talk*, *wipe*, *type* which will, when added to the array, *require* the analysis of *w*, *t* as morphemes in the example given above as motivation for the development of the method of arrays.

square based on *beats*, since it requires that both *breaks* and *beating* appear on the same row as *beats*. Or consider the following example from Modern Hebrew which is perfectly parallel to the Classical Arabic square cited above: *arag*, *oreg*, *halax*, *holex*. As in the Arabic case, we would like to "discover" in this example the discontinuous consonantal root and the discontinuous vowel pattern. Furthermore, whatever order is assigned to root and vowel pattern, it must be the same for both *arag* and *halax*. But the suggested revised convention will assign different orders in the two words, thus needlessly complicating not only the morphology, but also the syntax. In cases where squares containing "prefixes" as well as squares containing suffixes must be introduced into an array (e.g., English articles) the problem of finding some systematic convention which will permit proper construction of arrays limited to two dimensions is even more serious. It is not at all obvious how these difficulties can be corrected without other unwanted consequences. Note, incidentally, that in the Hebrew example just cited, morphological and syntactic considerations indicate that a "zero" morphophoneme precedes the initial vowel in *arag*, *oreg*, thus avoiding the difficulty. But the linguist whose aim is to develop step-by-step discovery procedures cannot allow himself the use of this insight at this point in his analysis.

There are other difficulties not involving discontinuities. The method of arrays is so designed as to allow the first and the last morpheme of a sequence to be segmented off. In the case where the sequence is in fact bimorphemic, this causes no difficulties. In all other cases, however, difficulties abound. Recall that given an arbitrary sequence of phonemes, method A is to be applied to determine whether it is segmentable; then method B (the method of arrays), to determine where the boundaries occur; then method A is to be applied to the remaining segments, followed by method B, etc., until all boundaries have been introduced. But the first application of method B to "boys like dogs" will detach the morphemes *boy* and the final *s* of *dogs*, leaving the sequence "s like dog" to be analyzed in the next step. But to this sequence no "meaning" can be attached at all, so that it will fit into no squares and method A cannot be reapplied to it. Attempts to modify the method of arrays so that it will recognize the main constituent break (this is apparently what is required here) must consider the difficulty posed by such sequences as "look it up," where no single boundary leaves two segments, each of which can be assigned a "meaning" so that analysis can proceed. It might be thought that this problem can be avoided at least in part by beginning the analysis with short utterances and analyzing long utterances in terms of them. This may be what Greenberg has in mind (cf. p. 27). But as we pointed out above, each utterance must

be analyzed independently, as these methods are now stated. No way has been offered for analyzing long utterances in terms of already analyzed short ones. Nor is it at all obvious how this can be done in general, *if* we insist on remaining within the limits of the "procedural" approach.

Greenberg's proposals for morphemic analysis are, to my knowledge, the most complete and detailed of their kind.[21] The aim of his methodological research is to provide a mechanical procedure of a simple, step-by-step nature, by which the optimal morphemic analysis will be reached in any arbitrary language, with no necessity for considering the effects of this analysis on the formulation of other parts of the grammar which are to be discovered by subsequent procedures. Since it seems clear that such a goal cannot be achieved by purely distributional procedures, Greenberg introduces semantic considerations very basically. It appears, however, that these do not assist in any substantial way. (We must of course be careful to avoid the temptation of using the word "meaning" in such a loose way that the higher-level syntactic function of inflections is called "part of their meaning"—this common but unfortunate usage simply obscures the fact that the methods under discussion fail to be "procedural" in the required sense, and confuses both semantic and syntactic studies to such an extent that progress in either is impossible.) Using meaning freely, we are about as far as we were before from having anything resembling a mechanical procedure for discovering morphemes. And we have, furthermore, paid an enormous price, in two ways. First of all, grammatical theory must now be based on some such notion as "sufficient correspondence in meaning variation" as a primitive term. Secondly, any hope of developing linguistic theory to the point where it can incorporate semantic studies must be given up. Study of meaning is not possible unless it is based on a clear and sophisticated understanding of grammatical structure. Consequently a linguist who is concerned primarily with procedures of analysis will have

[21] Proposals of comparable care and detail have been given by Harris (*Methods*, chapters 12–16) and Charles F. Hockett ("A Formal Statement of Morphemic Analysis," *Studies in Linguistics* X (1952), 27–39). But these approaches are essentially different in character in that they do not attempt to use meaning and they do introduce considerations of overall complexity of grammar into the choice of morphemes. Consequently neither approach is "procedural" in the sense of Greenberg's attempt, though this fact has often been overlooked. In this connection, cf. Chomsky, *Syntactic Structures* (The Hague, 1957), p. 52, fn. 3 and §6.2 for discussion of these papers. I doubt that even these approaches rely sufficiently on higher-level considerations in choice of morphemes.

A different kind of approach has been developed by Harris in "From Phoneme to Morpheme," *Language* XXXI (1955), 190–222. This is again a non-semantic, procedural approach, but it is intended only as a procedure which will provide a mass of preliminary information on which a morphemic analysis can be based. It cannot be depended upon to yield the optimal analysis directly, as is clearly stated in §§0.1, 5, 7.

eliminated the possibility of a systematic study of meaning by his very choice of a primitive term for grammatical analysis. Linguists who consider the study of meaning to be an important part of their field, at least potentially, will therefore have to reject such an approach. Though naturally one cannot safely predict the results of future research, it seems to me reasonable to suppose that the only hope for developing either grammatical or semantic theory lies in abandoning the search for simple, mechanical discovery procedures for grammars, permitting instead full-scale reliance on syntactic considerations and considerations of overall complexity of grammar in the choice of a particular morphemic analysis.

Exactly how much will we have given up, if this search is abandoned? As was pointed out above, the methods of the kind here under consideration are of such incredible complexity that even if they were somewhere near successful, no one would seriously consider the possibility of actually applying them in discovering a grammar. Furthermore, if the researcher gains a sudden and useful insight into the structure of a language, he will surely not reject it because it was not arrived at by any procedure. Hence the use of these procedures can only be in justifying a grammar somehow arrived at. But it is a rather peculiar idea to require that justification of a grammar must take the form of a demonstration that this grammar could, in principle, have been arrived at by applying direct mechanical procedures to the data, procedures which are, furthermore, nearly unique in the result they produce and of quite general applicability. Such a justification of systems as abstract and theoretical as grammars has never been thought possible in any other field, and there seems to be little reason to impose this very extreme condition on justification in linguistics, at least in this very early stage in its development.

The remainder of chapter II deals with the definition of 'word'. The problem faced here is that of devising a procedure which will determine which of the morpheme boundaries (assumed known) are word boundaries. Again, this discussion contains new and interesting ideas, but is marred by inadequacies of formulation that make it difficult to determine what the ultimate importance of these ideas will be, when properly clarified.

We define a *morph substitution class* (MSC) as a restricted class of morphs ('restricted class' is defined and discussed above, p. 205). Thus an MSC (like any restricted class) is defined relative to a fixed context.

A sequence $M_1 \ldots M_n$ of MSC's is called a *thematic sequence* if there is an MSC distinct from each of $M_1, \ldots, M_n$ for which the sequence $M_1 \ldots M_n$ is always substitutable. The intention of this definition is to include compounds and derived forms (e.g., *duckling*) as thematic sequences. Note that a thematic sequence must consist of more than one MSC.

A *nucleus* is either a thematic sequence, or an MSC which is not part of a thematic sequence.

A *word boundary* is a boundary between nuclei at which nuclei can be inserted indefinitely often (i.e., an infinite number of insertions are possible) provided that (a) the inserted sequence does not begin with the nucleus following the boundary and end with the nucleus preceding the boundary, and (b) that the insertion does not change the restricted class of the morphemes preceding and following the boundary.[22]

There are several difficulties here. A first group has to do with the notion 'restricted class' which appears twice. Note, first of all, that condition (b) in the definition of word boundary is empty. As 'restricted class' has been defined, no insertion can change the restricted class of the morphemes bounding the point of insertion. The condition was given to eliminate the possibility of inserting "-ing the other animals spar-" in the sequence "the farmer kill — ed the ugly duckling." It is claimed that this insertion changes the restricted class of *kill*, since this class now, but not previous to the insertion, contains *take*. But actually the example merely shows that *kill* and *take* were not in the same restricted class in the original sentence. Perhaps what is meant in condition (b) is 'contextual class' (see above, p. 205), since the contextual classes are changed by the insertion. A further confusion is caused by the choice of example here. Since "-ing the other animals spar-" is not a sequence of nuclei, in the intended sense of nucleus, no condition at all is needed to eliminate it from consideration, as 'word boundary' is defined without condition (b).[23]

It also seems unlikely that MSC's should be taken as restricted classes. The MSC's of the examples are clearly not restricted classes. Thus in the expression "the singer broke the contract" (p. 29), it is not the case that *mine* belongs to the restricted class containing *sing*, since insertion of *operate* before the suffix /ər/ (*-er*, *-or*) permits insertion of *mine* but not *sing*. Furthermore, it is certainly not the case that the class of elements determined by an array will in general be a restricted class, as here claimed. It is not too clear how MSC should be defined for the purpose of this construction. This makes it difficult to evaluate the effectiveness of the further definitions, since these depend heavily on that of MSC.

It would appear, however, that as long as MSC's are defined relative to context, certain difficulties are likely to arise in connection with the

[22] Notice, incidentally, that we cannot have a *procedure* based on the possibility of unbounded insertion.

[23] From other examples it is clear that the definition of 'word boundary' should be revised to involve indefinitely repeated insertion not, as stated, of single nuclei, but of sequences of (one or more) nuclei.

definition of 'thematic sequence'. For one thing, it is not clear what is the force of the word "always" in the definition, in case the context is fixed. It seems, furthermore, that the MSC containing *a* and defined by the context "—farmer killed the ugly duckling" may be replaced by "an (or each) old and rather well-dressed" (which is composed of MSC's presumably distinct from that of *a*), but we would not want to consider this a thematic sequence. Similarly, the MSC containing *us* in "he saw us" can be replaced by "the man," "the very old man," etc., and even, if MSC's are defined relative to a fixed context, "the man coming," etc. These too cannot be regarded as thematic sequences. If, for example, the phrase "the very old man" in "he saw the very old man" is regarded as a thematic sequence, then the words of this phrase will not be nuclei and word boundaries will not be properly introduced. In the case of the MSC of *us*, it would appear that these difficulties will arise even if MSC's are defined independently of context. However, the relevance of these comments depends on the exact way in which MSC is to be defined.

Perhaps 'thematic sequence' is to be understood in a way such that nothing which contains a thematic sequence qualifies as a thematic sequence. In fact, the examples given all bear out this interpretation. Furthermore, since every thematic sequence is a nucleus, some such interpretation as this will be necessary if the assertion (p. 28) that there are no more nuclei in an utterance than morphs is to be maintained.

Clearly a somewhat more careful formulation of this proposal is necessary before it can be properly evaluated. Notice should also be taken of certain more general difficulties that stand in the way of any attempt to base the definition of 'word' on possibility of insertion. There are many instances of sequences of words where unlimited insertion is not possible, e.g., once again, such phrases as "look it up." It may be that the notion 'word' must be defined relative to a limited class of particularly simple sentences, perhaps even a class which can be delimited only in terms of a full-scale syntactic analysis.

Despite the many defects in these formulations, many of the underlying ideas merit careful and continued study. If rigorous procedures can in fact be found, such attempts at rigorous statement are the only way to find them. If not, these attempts will still have been valuable because of the insight into language structure that results from a demonstration of the limitations of certain methods of approaching it. Failure to attempt rigorous statement can, furthermore, lead to a serious overestimation of the actual precision and effectiveness of linguistic methods.[24] Greenberg's

[24] The methods may be reasonably effective in the hands of a human investigator, but this of course tells us little about the actual scientific status of linguistic methodology.

work makes a definite contribution towards raising the level of methodological discussion. It is much easier to find defects in the methods he has developed (and, a fortiori, those that have been developed by others, since, as mentioned above, Greenberg's proposals seem to be the most carefully worked out of their kind) than to try to modify them to avoid these difficulties. Careful study of Greenberg's proposals, or any others of the same type that I know of, leaves little ground for the belief that linguists are very close to being able to formulate precise procedures which will, in a mechanical, step-by-step manner, lead to the optimal grammatical description for an arbitrary language. This is hardly surprising. It would be strange indeed if it were possible to develop methods for discovering something whose nature and structure is so complex and still so little understood as a grammar of a language.

In summary, the major contribution of these theoretical chapters lies in their emphasis on the necessity for rigor and on the relevance of mathematics to linguistics. The major weakness is an exaggerated (and widely shared) faith in the actual achievements of these (and perhaps any) procedures for setting up elements, and in the significance for linguistics of certain familiar mathematical ideas. These judgments are not mutually contradictory. There are weaker aims for linguistic theory than that of developing discovery procedures, and for these, rigorous formulation is essential. And although purely mathematical study of the abstract structure of grammars and linguistic systems may indeed prove to be highly important, it is not obvious that anything of substance will be achieved by simply borrowing and applying familiar and well-studied mathematical notions. It may be that the empirical motivations that lead linguists to undertake the study of abstract systems are sufficiently unique to require the development of new and relatively unexplored areas of mathematics. This has often been the case in other fields and it would not be too surprising to find out that it is true in linguistics as well.

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SHELOMO MORAG, *Ha-šva be-hagiiatam šel bne-Teiman* [The Šĕwā in the Traditional Yemenite Pronunciation of Hebrew]. 56 pp. Jerusalem, 1957 (Reprinted from *Lšonénu*, vols. 20 [1955/56], 21 [1956/57]).

Reviewed by HAIIM B. ROSÉN

The study under review is a chapter extracted from a more comprehensive work;[1] this book, in turn, forms part of an extensive research project the author has been working on for several years and which will probably occupy him for many more to come: the recording and assembling of oral traditions of Jewish communities in the reading of (Biblical and post-Biblical) Hebrew and Aramaic texts. The publications we have so far had from the material collected[2] show how much a thoroughly trained phonetician can contribute to a field of studies hitherto not really professionally examined.[3]

In a way appropriate to a study of a spelling pronunciation, Morag devotes each portion of his work to one single type of grapheme;[4] the present series of articles studies what is commonly known as šwa. The question posed is: How did Yemenite Jews[5] materialize[6] the šwa?

Any student of "familiar" Hebrew grammar would now expect the author to arrange his material in two major units: 1. šwa mobile, 2. šwa quiescens (see below); but this is precisely what cannot be done in this case and at this juncture.

There is a commonplace doctrine about the conditions in which a written šwa sign is made audible as a vocalic emission of voice (šwa

[1] *Yemenite Hebrew Pronunciation*, in print as a publication of the Hebrew Language Academy.

[2] Those so far published include: "Studies in the Living Tradition of Post-Biblical Hebrew as Preserved by Jewish Communities" (in Hebrew, with English summary), *Tarbic* XXVI (1956/57), 4–16; "The *pāʿēl* and *hitpāʿēl* Verbal Stems," *ibid.*, 349–356; "Babylonian Aramaic in the Traditions of the Jewish Communities," in *Summaries of Papers Read to the Hebrew Language Section of the Second Congress of Jewish Studies*, Jerusalem, 1957, pp. 2–3; etc.—Cf. also his paper, "A Special Type of Evolution," in *Proceedings of the 8th International Congress of Linguists*, Oslo, 1958, pp. 425–428.

[3] A survey of earlier, superficial attempts at the same problems is given on pp. 41–43, 48–49.

[4] On the justification of this term with respect to the study under review see below, p. 222.

[5] Informants were residents of Israel of Yemenite origin and of traditional skill as "readers."

[6] The term *mimuš* 'materialization, réalisation' is deliberately used by Morag throughout the study instead of the less professional *mivta* or *hagiia* 'pronunciation'. (The latter, however, appears in the title.)

mobile) and about the conditions where the same sign (šwa quiescens) is a mere graphic indication of such things as a fictitious absence of a consonant cluster, of an absolute final consonant, of a syllable boundary. But these conditions—clearly and simply as they are set forth in any of the handbooks of classical Hebrew grammar[7]—are nothing but an idealized and much too simplified picture of "masoretic" Hebrew, based upon the rules of those of the manifold medieval grammatical treatises (erroneously conceived of as purely preceptive) that have become accepted among European humanists.[8]

On pp. 4–20 the author shows that this idealized picture is not in keeping with reality as regards the Yemenite readers of Hebrew;[9] as a matter of fact we know that it hardly fits any of the Jewish communities with complete exactitude. Morag finds that consonant clusters are avoided in the positions *CCC* and *V́CC* and a vocalic emission is produced between the last two consonants of these sequences (normally [CCVC, V̆CVC]), but that initial clusters C_1C_2 are admitted ([tʕu:ðo·], p. 23; even at morpheme boundaries: [l-ʔöθöθ], p. 24), if C_2 is pharyngeal ("laryngal") and C_1 is not. He furthermore finds that although "a šwa is mobile if the vowel preceding it stands in an open syllable," certain syllables we would expect to be open admit a consonant cluster (at the beginning of the following syllable?), e.g. [so|mḥu:] (or [som|ḥu:]?) 'they rejoiced' (p. 24), generally thought of as [so|mŭ|ḥu:] (i.e. in ordinary transliteration *śå|mᵊ|ḥu:*).[10]

These examples suffice to show the limitations of categories of Hebrew phonetics that traditional scholarship has established. But still more

[7] Gesenius-Kautzsch (trad. Cowley), *Hebrew Grammar*, ₂1910, §10c, i; Joüon, *Grammaire de l'hébreu biblique*, 1923, §§8b, c, 28b, c; only Bergsträsser, *Hebräische Grammatik*, 1918, §10e, h, gives some account of the facts as they are. The latest handbook (Lambert, *Traité de grammaire hébraïque*, 1938) has consequently chosen to be noncommittal: "Pour les autres cas, où le šewâ est au milieu du mot, c'est l'étude des formes grammaticales qui en indique la nature" (§39).

[8] Cf. O. Jespersen, *Language*, London, 1922, p. 21; L. Kukenheim, *Contributions à l'histoire de la grammaire grècque, latine et hébraïque à l'époque de la renaissance*, Leiden, 1951, pp. 124ff.—A factor which encouraged the ready acceptance of just those rules may have been their comparative convenience for purposes of Semitic comparison on 19th-century lines.

[9] To be exact, the instances assembled by Morag are arranged according to Biblical (specimen: *Isaiah*, ch. 7–8) and post-Biblical language, and some minor differences are detected. We shall here neglect the details of post-Biblical Hebrew, as the phonemics and morphophonemics of this *état de langue* have hitherto been the object of only negligible linguistic study, so that the author's achievement in this respect, albeit more novel, is here of lesser impact upon accepted notions of Hebrew grammar.

[10] I.e. thus taught by accepted scientific grammars. The earliest Hebrew grammatical tradition usually considers the consonant cluster in this morphological type (*qå̆ṭlå*) as admissible. Note, in favor, of Morag's syllabification, that the cluster involved is of the

stimulating facts become evident when we turn to a quantitative and qualitative examination of the mentioned vocalic emission (pp. 21–43): if $C_1C_2V_2$ is materialized $[C_1V_1C_2V_2]$, the quality and quantity of $[V_1]$ may briefly be schematized as follows:

	C_2 = pharyngeal	$C_2 = \dot{I}$	Free Allophone
C_1 under secondary stress or musical prominence	$[V_1] = [V_2]$[11]	= [i]	= [a]
Otherwise	$[V_1] = [\breve{V}_2]$[11]	= [ĭ]	= [ɛ̆]

The free allophones[12] of šwa thus coincide with those of the Yemenite phoneme representing traditional /a/ (pataḥ) and /ɛ/ (segol), and the area of neutralization of the contrast ("overlapping") of that phoneme and zero[13] (šwa) is thereby defined, i.e. "šwa is mostly pronounced as if it were a pataḥ."[14]

Morag has succeeded in showing throughout his study that the facts revealed by him are to a considerable degree compatible with the teachings of medieval grammarians, especially those that were read and copied in the Yemenite community. He allows the assumption that the readers on the whole followed the rules they had learned and that, in particular, the distinction between presence and absence of a vocalic emission of sound was caused by the orthography of those Yemenite manuscripts of Hebrew texts where only the "mobile" šwa is written and no graph exists for what we would consider as šwa quiescens.

But here is precisely the place to stress that the earliest medieval grammarians[15] in no way showed a preceptive attitude: they were descriptive grammarians who treated Biblical Hebrew morphology through the reading habits of the authoritative members of their respective communities.

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initially admitted "non-pharyngeal–pharyngeal" type. A treatment of admissibility of clusters (whether initial or medial) according to the articulatory nature of both components is a novelty of principle introduced by Morag into the grammar of Classical Hebrew.

[11] Cf. L. S. Hultzén, "Free Allophones", *Language* XXXIII (1957), pp. 36–41.

[12] There is a parallel rule in "accepted" grammar only for the case that *V* is of minimum duration (šwa compositum).—Ordinarily, secondarily stressed or liturgically prominent vowels are, in Yemenite tradition, of longer duration, but not qualitatively different from their unstressed counterparts, except the pair [a] : [ɛ̆] (pp. 38f.).

[13] I. Garbell, "The Phonemic Status of Šwa and the *Ḥăṭefim*," in *Summaries* etc. (see note 2), p. 12.

[14] In the other cases (presence of pharyngeals) the pronunciation of unstressed šwa corresponds to that of šwa compositum (pp. 44ff.).

[15] And above all the author of *Diqdūqei-haṭ-ṭᶜāmīm* that still remains the principal authority for the questions under examination here and that agrees with many of the features revealed by Morag.

Morag (although he does not expressly say so) seems to be perfectly aware that he has put himself in their place and that his description of the "pronunciation" of any one community is tantamount to a "Hebrew Grammar" of medieval scope.

But can the "phones" which are recorded, analyzed and categorized be properly called "realizations" of phonemes? The treatment of this theoretical question is conspicuously absent from Morag's articles. The point is that where a grapheme has several variant pronunciations depending on the context, each of these pronunciations, even if they are in complementary distribution, resembles a separate *Lautabsicht*, i.e. a phoneme rather than an allophone. This is borne out by the fact that these pronunciations had to be taught as such, since the conditioning role of the phonic context was not automatically operative in the speech habits of the readers who supposedly uttered each single item of taught "pronunciation" in accordance with the allophonic variations of their vernacular. For example, when Biblical Hebrew /ʕ/ was a pharyngal, it had an allophone [^{a}ʕ] in final syllables after a long vowel; when later readers of Biblical Hebrew merged /ʕ/ with /ʔ/ and zeroed both as finals, the reading of the graphically represented [^{a}] (patah furtivum) subsisted in spite of the absence of any conditioning factor. This new /a/ has in turn, for some Israeli speakers, the allophones [wa] after *ó* and *ú*, [ya] after *í* and *é* (e.g. [lišmówa] /lišmoa/ < [lišmo^{a}ʕ]), and these glides [w] and [y], readily explainable as they are,[16] can in no conceivable way be connected with the phonic properties of either /ʕ/ or even its successor, final zero.

As regards the graphemes themselves, we must not underrate the importance of the fargoing subphonemic narrowness of at least the Tiberiad notation of Hebrew. It is a long path through this almost impenetrable labyrinth of layers of analysis (which may in due course turn out to be layers of a diachronic phonology!) to the phonemics of Biblical—let alone post-Biblical—Hebrew. Morag himself has made no attempt to group his phones into phonemes: he could not have easily done so, I think, as these are not properly materializations of "phonemes." But he was obviously aware of the fact that such phonemes would not have been identical with those established by any phonological school in an analysis of the Gesenius-type Biblical Hebrew.[17] Let us dwell on a few details:

[16] Cf. C. Rabin, "La Chute de l'occlusive glottale en hébreu parlé et l'évolution d'une nouvelle classe de voyelles," *Comptes-rendus du Groupe linguistique d'études chamito-sémitiques* III (1940), pp. 77–79.

[17] Z. S. Harris, "Linguistic Structure of Hebrew," *Journal of the American Oriental Society* LXI (1941), pp. 143–166; J. Cantineau, "Essai d'une phonologie de l'hébreu biblique," *Bulletin de la Société de linguistique de Paris* XLVI (1950), no. 1, pp. 82–122.

1. The overlapping in the Yemenite tradition of šwa compositum (an allophone of šwa after a pharyngeal) and the šwa mobile (when preceding a pharyngeal) demands a specific formulation of the distribution of the allophones of šwa, a formulation which takes into account both conditioning features at one time and discards the *ḥăṭēfīm* (šwa composita) as a particular category (cf. p. 46).

2. Establishing certain allophones of šwa as occurring simply in "the vicinity" (see under 1) of a pharyngeal and of another allophone ([i]) as occurring in "the vicinity" of *j* may show that these are the same vowels as are produced in much the same conditions in the final unstressed syllables of the segolate type words ([a, i] in contact, respectively, with a pharyngeal and *j*). This would corroborate our assignment[18] of those unstressed vowels to zero, i.e. to the same phonemes as the various allophones of šwa.

3. An interesting attempt could be made to give a phonological "explanation" of the free allophone (cardinal value) of Yemenite and of properly "Sephardic" šwa. The respective vowel systems may be sketched as follows:

	Yemenite[19]		Sephardic	
	Not rounded	Rounded	Front	Back
High	*i*	*u*	*i*	*u*
Mid	*e*	*ö*	*e*	*o*
Low	*a* ([a], [ɛ])	*o*	?	*a*
	< Bibl. Heb. *a, ɛ*	< Bibl. Heb. *â*	< Bibl. Heb. *ɛ*	< Bibl. Heb. *a, â*

As an uncharacterized (neutral) allophone, [ɛ], was included in the Yemenite /a/-phoneme, an absorption[20] of the neutral actualization of vocalic šwa was possible;[21] such a possibility however did not exist for the Sephardic system, where the /a/-phoneme had arisen out of the merger of two other Biblical Hebrew phonemes.

What is the nature of the multitude of phonemic systems found as a variety of "reading traditions" of Jewish communities? Is it a variety of dialects derived from a common source—"Biblical Hebrew"?[22] Does "Biblical Hebrew" in this context mean the Tiberiad text—or is this text

[18] Cf. the present reviewer in "Remarques au sujet de la phonologie du l'hébreu biblique," *Revue Biblique* LX (1953), pp. 31–36.

[19] Modified from Morag, p. 21.

[20] A. Martinet, *Économie des changements phonétiques*, Berne, 1955, p. 91.

[21] Morag mentions (p. 21) that the šwa graph frequently represents Arabic [a], [ᵃ], [ā], when the Jewish variety of Yemenite Arabic is written in vocalized Hebrew characters.

[22] There are indications that phonic features like those revealed by Morag are of considerable antiquity: Septuagint spellings like *Ιοαακ* (*jiṣḥăq*), *Βαλααμ* (*bilʕăm*),

rather one "dialect" of tradition? Morag has touched upon these points and on the "stemma" questions involved elsewhere, but it is clear already that studies like his are liable to revise the "accepted" notions of Hebrew phonology,[23] and it is also clear that in speaking of Biblical Hebrew, we shall be wise to discard "phonemics" and limit ourselves to "graphemics," unless and until more studies are written with like accuracy and profoundness as the one reviewed here.

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Βεσελεηλ (*bṣal?ēl*), *Αναμεηλ* (*hanam?ēl*), etc., are consistent with the rules mentioned here for šwa preceding a pharyngeal; cf. also R. Helbing, *Grammatik der Septuaginta*, Göttingen, 1907, p. 27; C. Har-Zahav, *Dikduk 'ivrii*, vol. II, part I, Tel-Aviv, 1950–1951, p. 143; see also Morag's lecture quoted in footnote 2, and (for hexaplaric evidence) his own footnote 3 on p. 34.

[23] E.g. the golden rule that unstressed vowels are long in open and short in closed (unless final) syllables (" · [i.e. length] is to be considered a consonant," Harris, *op. cit.*, p. 149; cf. Cantineau, *op. cit.*, p. 111f.) can no longer be upheld.

MEDEIROS, MANUEL FRANCISCO DA SILVA DE, and collaborators, *Dicionário Técnico Poliglota: Português, español, français, italiano, English, Deutsch: Ciências puras e aplicadas, Medicina e Farmácia, Engenharia e Arquitectura, Ciências militares, Transportes e Telecomunicações, Indústria e Comércio*. Eight volumes. xlvii+1184 pp., 1623 pp., 1603 pp., 692 pp., 638 pp., 623 pp., 662 pp., and 692 pp. Lisbon: Gomes e Rodrigues, Lda., 1957. 2nd printing.

Reviewed by JOHN L. GRIGSBY, JR.

Sr. Medeiros (B.Sc., Mech. Eng., Tri-State College, U.S.A.) was assisted by a group of thirty-three collaborators who are mostly Portuguese and mostly situated in the educational institutions or industries of Lisbon. The dictionary is arranged in the following manner: The first three volumes contain the dictionary proper, i.e., the main alphabetical list of Portuguese words and terms (68,313 of them), each followed by their equivalents in Spanish, French, Italian, English, German, and sometimes Latin. Each of the five remaining volumes is an index for the languages other than Portuguese, and, at the back of each of these is a Latin index. It is the identical Latin index in each volume. The indices are keyed to the main alphabet by means of code numbers. If a user looks up the word *Einpacker* in the German index, he will find "NC" (*número do código*) 03450. By looking at the backs of the first three volumes where the contents are clearly

indicated, he can determine that 03450 is in volume I. He follows "NC" 03, which is printed at the top of each page, to the page containing 03: 450. (The latter part of the number is printed in the margin of the page.) The entry found, the word *Einpacker* may be translated into any or all of the five other languages. In some entries the user will find, in addition, the Latin translation, e.g., 00794, *abóbada da faringe*: *Fornix pharyngis*; in others, a chemical formula, e.g., 02703, $SnCl_4H_2$. The Latin term or chemical formula, when they occur, appear second in the article, just after the Portuguese, thus saving the reader the task of searching throughout the entry.

The introduction, forty-seven pages in length, contains a preface, a coding system for telegraphic correspondence, and a list of abbreviations; all have been tr.nslated into the six modern languages of the *Dicionário*. In the preface (apparently written by Sr. Medeiros) the editor notes that two types of dictionaries exist today, each of which represents an extreme. One type is the short, specific lexicon which is often compiled for a particular machine or instrument and exported with it to another country. This type is too restricted, too technical. The other type of dictionary is the bi-lingual dictionary of general scope that lacks many technical terms. Sr. Medeiros has endeavored to bridge the gap between the two extremes. "Ainsi l'œuvre que nous présentons . . . a un but modeste et positif: elle permet de résoudre les nécessités linguistiques de tous ceux qui ont besoin de comprendre les textes modernes spéciaux rédigés dans six des principales langues de culture technique et scientifique surtout dans les parties où échouent les dictionnaires bilingues" (I, xxii).[1]

The telegraphic coding system which appears in the introduction is a curious and unusual feature. It provides a method of transferring the *Dicionário*'s code numbers into code letters. The principal part of the system is a telegraphic coding chart. How to use the chart is briefly explained in each language: The sender of a cable or telegram determines the code numbers of the expressions he desires to relay. By means of the chart he changes each five-digit number into a five-letter unit. The receiver of the telegram procedes inversely. He re-transfers the letters to numbers and may then decipher the message into any one of the six languages. The *raison d'être* for the coding system escapes this reviewer. Why transfer the numbers to letters? The fact is that a message consisting of a series of numbers—the cable companies are quite familiar with such messages—is likely to be more accurate than one composed of letters. When numbers

[1] Why is the preface in its English translation only a half-page in length, while the Portuguese, Spanish, French, Italian, and German occupy two full pages? Does this indicate that the dictionary was not particularly destined for English-speaking users?

are sent over the cables of Europe and America they are repeated at the end of the message for verification purposes.

The list of abbreviations on the last four pages of the introduction could have been more conveniently placed if printed on the inside sheets of the cover (which have been printed with a decorative but non-utilitarian design).

On first glancing at the dictionary, users may be sceptical of its value because of the preponderance of the letter *a*. Volume I (15,566 entries) contains words and expressions beginning with *a* alone. Volume II (24,862 entries) contains letters *b* through *f*; and volume III (27,885 entries) contains the rest of the alphabet. Aside from the fact that volumes II and III have more pages and more entries, there are three less obvious reasons why the letter *a* has more space devoted to it. It is not because the editors were more ambitious at the outset of their project than at the end. In the first place, the alphabet which was chosen as a base, Portuguese, is heavily laden with words beginning with *a*, while very few words begin with *k*, *w*, *x*, *y*, or *z*. A second reason is that a number of variants exist in Portuguese that begin with *a*. The editors have listed such variants under *a* rather than another letter of the alphabet, e.g., *(a)condicionar*, *(a)caju*, *(a)boiar*, *(a)loquete*. A third reason is that synonyms for words beginning with *a* are listed under the *a*-word. As the synonyms occur later in the alphabet, they are followed only by a cross-reference to the *a*-word. For example *ácido cumarílico* (02726) is followed by the alternate expressions *metilóico-2 cumarona* and *fenofurfuranometilóico* plus the entire list of foreign equivalents. The expressions *metilóico-2 cumarona* and *fenofurfuranometilóico* are alphabetized later under *m* and *f* but instead of a six- or seven-line entry of equivalents, the code number alone follows, which directs the user back to *ácido cumarílico*. Since this procedure is continued throughout the dictionary, the number of terms listed under each letter tends to diminish as one goes toward the end of the alphabet.

The editors have made painstaking efforts to discriminate between meanings. Discrimination is shown, not within entries, but between entries. Expressions of only slight shades of difference appear in separate entries, a practice which leads to some overlapping. For the English expression *to arrive safe and sound* there are two entries, 22895 and 22913. The former translates Portuguese *chegar a salvamento*, the latter *chegar são e salvo*. The equivalents in all the other languages are almost identical in both 22895 and 22913. The need for two entries to express one idea seems slight. Would not one entry which would include all variants have sufficed? In another case, there are again two English entries which have the same meaning; 53192, which reads *please*; *begging you to have the kindness to*

and 57362, which reads *please*; *be so kind as to*; *have the goodness*. Unfortunately, the differentiation in these English expressions helps the English-speaking user but little to distinguish between the shades of politeness in the Portuguese *pedindo-lhe queira ter a bondade de* (53192) and *queira*; *sirva-se*; *tenha a bondade* (ou *complacência de*) (57362).[2]

A second method of discrimination is the inclusion of a label which is placed immediately after the lead word of an entry, e.g., **cúmulo** (*Meteor.*), **ansa lenticular** (*Anat.*), **perlasso** (*Quím.*), **rutáceas** (*Bot.*).

For some concepts the editors have listed quite a number of variants. They do not note whether or not the variants are equivalent but we may assume that they are since this would be consistent with the practice of not discriminating within an entry. An example is entry 03819: *crude sugar*, *raw sugar*, *coarse sugar*, *moist sugar*, *muscovado*, *brown sugar* for the English; and for Portuguese: *açúcar em rama*, *açúcar bruto*, *açúcar mascavado* (ou *mascavo* ou *mascate*), *açúcar mulato* (ou *loiro*), *rama* (*de açúcar*). In other cases the editors make note of variants which are peculiar to certain countries, e.g., 22969: *chulipa*; *durmiente*, (Colombia) *polín*; *sleeper*, (U.S.) *crosstie*.

The dictionary contains many expressions that one would not expect to find in a technical dictionary. Some of these are *to hope that one's efforts will be successful* (05455); *to come to an understanding* (22897); *sausage* (22967); *blood-pudding* (22968); *to ask a favor* (53238); *to ask a question* (37791); *be convinced* (36055); *to ascend, to climb up, to mount* (13804). It contains many technical neologisms, e.g., *atomic energy* (33348), *atom-pile* (54105), *jet engine* (48986), *turbojet engine* (66234), *antibiotic* (10141), *artificial "fecundation"*—for *"insemination"*—(37942), *frequency modulation* (48372), *microfilm* (48040), *radar* (57480), *supersonic speed* (67103). It does not include, for example, *nuclear fission*, *ram-jet*, *acetate rayon*, *aureomycin*, *betatron*, *butadiene*, *chain reaction*, *cloud seeding*, *flame-out*, *psychosomatic*, *thermonuclear*.

In order to demonstrate fully and clearly the use of prepositions in the various languages, the editors have included entire expressions:

22911: **chegar** ***v.*** **no comboio** (***ou*** **trem**) das x . . . horas. 2 llegar *v.* en el tren de las x . . . 3 arriver *v.* par le train de x . . . heures. 4 arrivare *v.* col treno del x . . . 5 to arrive by the x . . . o'clock train. 6 mit dem x . . . Uhr Zuge ankommen.

[2] In the latter entry (57362), the conscientious attention the editors usually pay to grammar is absent. They do not show that the Spanish, French, and English terms are sometimes followed directly by an infinitive and sometimes by a preposition to introduce the infinitive. They indicate this only vaguely in the Portuguese because the closing parenthesis is misplaced. The former entry (53192) is clear.

22912: **chegar *v.* no vapor . . .** 2 llegar *v.* por el vapor . . . 3 arriver à bord du vapeur . . . 4 arrivare *o* giungere *v.* col piroscafo . . . 5 to arrive on *or* upon the s/s . . . 6 auf dem Dampfer . . . ankommen.

Verbs are indicated by *v.* but help in the conjugation of verbs in such expressions is not given. As for nouns, gender is not stated for the masculine singular forms in the Romance languages but is for the feminine and plural forms. English nouns are indicated as such only when they occur in plural form while in German the gender and number of all nouns are indicated. The editors show feminine nouns by the simple abbreviation *f.*, omitting an *s* for *substantivo* or *n* for *nome*, either of which would have been superfluous. Elimination of the period after *f.* (and, by the way, after *v.*) would have made the economy of space complete.

Clarity is assured in the interior arrangement of each entry by the repetition of words when variant expressions exist, e.g., 00794, Spanish *bóveda de la faringe*; *bóveda faríngea*. The repetition avoids the possible confusion arising from the practice of using shortcuts such as *b.*, or the sign ~ for *bóveda*. This is just one instance which demonstrates the editorial care exercised in the *Dicionário*.

It would be difficult to test the reliability of this dictionary without duplicating the efforts of all the contributors in Lisbon who participated in the task. A quick comparison with other polyglot dictionaries such as the *Handy Technical Dictionary* (London: "Disce" Publications Limited, 1949), *Pitman's Technical Dictionary* (London: Pitman and Sons Limited, 1928–1929), the Schlomann-Deinhardt *Illustrierte technische Wörterbuch* (Berlin and New York, 1906–1932) reveals only that there exist variations among technical dictionaries, but this is not a test of reliability for the *Dicionário*. Both the *Handy Technical Dictionary* and the *Illustrierte technische Wörterbuch* are reinforced with illustrations making each page a veritable testing ground for reliability. It is regrettable that the *Dicionário* contains not a single illustration or diagram. Using them, however, would have caused other sacrifices. Cost of publishing would have been much greater, possibly reducing the corpus of words included, possibly delaying or prohibiting the completion of the work. Alphabetical order would have had to be abandoned in order to cluster the pertinent terms around the illustrations.

An inherent inconvenience of this large dictionary is the necessity of handling so many volumes. There is no way to eliminate the constant referring to the main alphabet via the indices, unless one starts from Portuguese. In spite of the reward of having several languages at the end of each search, the manipulation of at least two volumes, and often four,

for every term renders the process tedious. Furthermore, when several identical words occur in an index, no distinction is made among them *in the index itself*. For example, if a user wants to find the word *elevator* (*of an airplane*), he must check *elevator* (13821) which is a "lift" or "hoist." He must see *elevator* (32552) only to discover that it is an anatomical term. If he looks up *elevator* (32553), he finds that it is again a "hoist." Finally, *elevator* (45041) turns out to be the aeronautical term he wants.

One work cannot fulfil—or has not up to now fulfilled—every lexicographical need. What criticism this review bears can be reduced to three points: first, that Russian was not included; second, that illustrations were not used; third, that to use the *Dicionário* one must manipulate too many large volumes. But the work of Sr. Medeiros and his staff attempts to satisfy a need for a dictionary which is neither too technical nor too general. In this, they have perhaps succeeded. Their lexicographical technique shows consistency, a quality which is surprisingly rare in bilingual and multilingual dictionaries. Typographical errors seem to be at a minimum.[3] Because the *Dicionário* includes much general vocabulary it can be used conveniently by students of Portuguese in the United States as a Portuguese-English dictionary, but it may find its greatest number of users among export-import companies engaged in a good deal of commercial correspondence in Western European and American countries.

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[3] The editors promise an appendix of corrections and addenda. Such an appendix was not a part of the copy which this reviewer saw. Perhaps it will appear separately.

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